NATURAL SCIENCE

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NOTES AND COMMENTS

Governments and Fisheries

The American Society of Naturalists has presented to President McKinley a strongly worded resolution, intended as a protest against the dreaded appointment to the Commissionership of Fish and Fisheries of a politician without scientific attainments. The matter has been warmly taken up by the scientific press of America, and scientific men all the world over will sympathise strongly with this protest. There is hardly a country nowadays that does not recognise to the full the importance of a truly scientific study of the sea and its inhabitants. If anything is to be done to improve the condition of the fisheries, all laws and practical suggestions are bound to have a scientific basis. But while we see every country of Europe investigating the conditions of its seas, or its lakes and rivers if it has no seas—while this course is being pursued by our colonies, and by the rising nation of Japan, and while America has hitherto been, and doubtless will continue to be, one of the leaders in this work, it is surprising as well as disappointing to learn that the Government of Egypt, in which our own country at present takes so large a share, should quite recently have thought it advisable to dispense with scientific control of their fisheries and to hand them over to an utterly inexperienced coast-guard department. We are in no way concerned with individuals, but we do not imagine that even the Egyptian Government will deny that in Dr J. C. Mitchell they had one who was, by his scientific training and practical experience, thoroughly qualified for the post of scientific and technical adviser to the fishery administration; and we are glad to be able to publish in this number a sketch of the fisheries of Egypt from the pen of that gentleman. But totally apart from the hardship to Dr Mitchell, in being deprived of an important post at five weeks' notice, seeing all his plans for the improvement of the Egyptian fisheries suddenly

cut short and his researches on the fish fauna of the Lower Nile put an end to, we must express our decided opinion, which we are quite sure will be the opinion of all our readers, that in taking this step the Government of Egypt is not behaving fairly to its subject population. With this population fish ranks higher than flesh, and an enormous saving to the scattered wealth of the country could easily be made in a very short time by a properly equipped department. Our position in Egypt will hardly be strengthened in the eyes of the world by such retrograde actions as that of which we now complain.

Two Views of Physiology

The Fullerian Professorship of Physiology at the Royal Institution was recently vacated by Professor Waller, since he was unable to obtain a laboratory, without which he declared neither the investigation nor the teaching of physiology could be prosecuted. This has not prevented Professor E. Ray Lankester from accepting the same chair. Professor Lankester, to judge from the first of his lectures on "The Simplest Living Things," now being delivered at the Royal Institution, holds broader ideas as to the meaning of the term physiology. He eases his conscience by interpreting it in its old sense, for which we now generally use the term biology. He is more than right, for there is no doubt that physiology to-day is becoming less the study of functions of specialised organs and more a study of the properties of living matter itself, as exemplified in the lowliest of organisms.

The view that we have just expressed was in fact strongly emphasised by Dr J. Loeb at the recent meeting of the American Society of Naturalists. His address is printed in the *Botanical Gazette* for January, and we hope our excellent contemporary will pardon us if we make copious extracts: "Living matter is a collective term for the quality common to all living organisms. Comparative physiology alone enables us to discriminate between the general properties of living matter and the functions of specific organs, such as the blood, the nerves, the sense organs, chlorophyll, etc. Nothing has retarded the progress of physiology and pathology more than the neglect of comparative physiology. Comparative physiology shows that secretion is a general function of all living organisms and occurs even where there is no circulation. Hence it was *a priori* false and waste of time to attempt to explain secretion from the experiments on blood pressure. Oxidations occur regardless of circulation, and it was *a priori* a waste of time to consider the blood as the seat of oxidation. Comparative physiology has shown that the reactions of

animals to light are identical with the heliotropic phenomena in plants. Hence it is a mistake to ascribe such reactions as the flying of the moth into the flame to specific functions of the brain and the eyes. Sleep is a phenomenon which occurs in insects and plants, and it would be waste of time to attempt an explanation of sleep on the basis of phenomena of circulation. The best interests of physiology and pathology demand that the systematic development of comparative physiology be one of the physiological problems of to-day."

Stereochemistry and Physiology

The chemical changes that take place in connection with life and the gradual organisation of living matter are phenomena that the physiologist of to-day is trying hard to connect. In his attempt he may be aided, says Dr Loeb, by two series of facts: "(1) The fact that phenomena of fermentation lead to an increase in the number of molecules and thus bring about an increase of osmotic pressure in the cells, this increase of osmotic pressure being the source of energy for the work of growth; (2) the facts of heteromorphosis, *i.e.*, the possibility of transforming in certain animals one organ into another or substituting one organ for another through external influences, such as gravitation, contact, light, etc."

"The exact and definite determination of life phenomena which are common to plants and animals is only one side of the physiological problem of to-day. The other side is the construction of a mental picture of the constitution of living matter from these general qualities. In this portion of our work we need the aid of physiological chemistry and especially of three of its theories—stereo-chemistry, Van't Hoff's theory of osmotic pressures, and the theory of the dissociation of electrolytes. We know that the peculiar phenomena of oxidation in living matter are determined by fermentative processes, and we venture to say that fermentations form the basis of all life phenomena. It has been demonstrated that fermentability is a function of the geometrical configuration of the molecule. *Saccharomyces Cerevisiae* is a ferment for such sugars only as have three, or a multiple of three, atoms of carbon in the molecule. Among the hexaldoses only ∂-glucose, ∂-mannose, and ∂-galactose are fermentable, while their stereo-isomers are not fermentable. But the influence of the geometrical configuration goes farther. Voit has suggested and Cramer has demonstrated that there is a far-reaching parallelism between the fermentability and assimilation of carbohydrates. Higher animals as well as yeast cells are able to form glycogen from such carbohydrates as are fermentable by yeast. The further development of these stereo-

chemical relations and their extension to proteids and nucleins is another of the problems of physiology which will contribute to the main problem, the analysis of the constitution of living matter."

Physiologists who may desire to take up the study of this fascinating but intricate subject may be glad to know that Messrs Longmans, Green & Co. have just published a second edition of Dr Eiloart's translation of Van't Hoff's work, "The Arrangement of Atoms in Space." This deals with the stereochemistry of carbon and of nitrogen compounds, and contains an appendix by Professor Alfred Werner of Zurich, on the stereochemical isomerism of inorganic compounds. This edition has been thoroughly revised and brought up to date, and contains a full index. The price is six shillings and sixpence. Recommendation of such a classical work is superfluous.

Osmosis and Animal Electricity

The passage of fluids through thin membranes, known as osmosis, deserves even more attention than it has hitherto received, since it enables us to understand many vital processes that would otherwise seem to be inexplicable on purely physical or chemical grounds. Again, to quote Dr Loeb, "Van't Hoff's theory of osmotic pressure permits an application of the law of conservation of energy to a class of phenomena to which this law was hitherto inapplicable, namely, the phenomena of growth, functional adaptation, secretion, absorption, and even pathological processes, such as oedema. The physiologists who thought that the blood pressure determined secretion could not understand why secretion took place under a higher pressure than the blood pressure. Comparative physiology shows that secretion does not depend upon circulation, and the theory of osmotic pressure indicates that the osmotic pressure in the cells is more than twenty times as high as the blood pressure. The work of secretion is done by osmotic pressure and not by blood pressure. A prominent physiological chemist has become a vitalist because he could not explain why the secretions differ from the blood from which he thinks they are formed. He overlooks, among others, the fact that the protoplasm possesses the quality of semi-permeability, which means that it allows certain substances to pass through and others not."

"The theory of the dissociation of electrolytes is of fundamental importance in the analysis of the constitution of living matter. Pharmacology will feel its influence most directly. Everything seems to indicate that the specific physiological effects of inorganic acids are due to the number of positively charged hydrogen ions in the unit of solution, and the specific physiological effects of alkalies to the negatively charged hydroxyl ions. But the universal bearing

of the theory of dissociation upon physiology will perhaps be best seen in the field of animal electricity. An active element of living matter in a state of rest is negatively electric to its surrounding parts. We may assume that an acid is formed in the active part, and that the passive parts are neutral. The positive hydrogen ions of the acid have a much greater velocity of migration than the anions. Hence the former will diffuse more rapidly into the passive tissue than the anions, and the active tissue will remain negatively charged."

Martian Morphology

What relief, after wading through the tenth text-book of zoology or the twelfth primer of geology, to open a parcel from Mr Wm. Heinemann, and to find that it contains that interesting romance "The War of the Worlds," by H. G. Wells! Our best thanks to Mr Heinemann for a really pleasant evening. Considering that the staple of an up-to-date novel is criminal psychology or sexual pathology, it is strange that we receive so few. But there is little of the advanced novel in this thrilling tale by our English Jules Verne. Everyone by this time knows the story of the attack on London by the strange beings shot from Mars: this we need not recapitulate. We are more interested in the anatomy and physiology of the Martians themselves.

Some of our readers may remember that in September 1894 we were led by the strange light seen on the southern edge of Mars (which Mr Wells explains as due to the casting of the huge gun), to speculate on the kind of beings, and especially the reasoning beings, that might have been evolved did protoplasm exist on the red planet. We did not enter into such details as the trained scientific imagination of Mr Wells enables him to do, but perhaps we do not lay too flattering an unction to our soul in supposing that our hints formed the basis of the novelist's more vivid conception.

A big, greyish, rounded bulk, about 4 feet in diameter, with an integument glistening like wet leather, a face, or rather a facial area with immense dark-coloured eyes devoid of brow ridges, no nostrils, and below them a V-shaped mouth with pointed upper lip, its wedge-like brim, unsupported by a chin, incessantly quivering and dropping saliva. And then below this, sixteen slender, almost whip-like, tentacles arranged in two groups, "since named by that distinguished naturalist Prof. Howes, the hands." At the back of its head or body is a single tight tympanic surface serving as an ear, the internal viscera consisting chiefly of a brain sending enormous nerves to the eyes, ear, and tentacles. The mouth opens not into a stomach, but into lungs, the heart and its vessels being

the only other internal organs. Such is Mr Wells' Martian, "a horror and a monstrous prodigy." The Martian, having no stomach, cannot eat, much less digest: instead he injects the living blood of other creatures into his own veins. He never sleeps, having no extensive muscular mechanism to recuperate. The Martians are without sex, and their young arise by budding. We are also interested to find that Mr Wells' Martians are, as we suggested, deaf "to what we hear, and realising their environment in ways inscrutable to us," although it must be admitted that in this point our author is not wholly consistent.

Another piece of inconsistency lies in the black smoke, which on page 145 is said to contain "an unknown element giving a group of four lines in the blue of the spectrum," whereas on page 298 the same element shows "a brilliant group of three lines in the green." This is a little fact that Mr Wells might have made certain of without any trouble, for, as many little details prove, he is well acquainted with various branches of science. It was, for instance, an ingenious idea to suppose the Martian machines constructed on the plan of human joints and muscles, while assuming the entire absence of that distinctively human invention—the wheel. There are no bicycles in Mr Wells' Mars.

Our Microscopic Allies

We believe it is not a new suggestion that the red colour of Mars is due to the red chlorophyll of the herbage, but the introduction of the Red Creeper and the Red Weed affords a detail not merely ghastly in itself, but, preparing the way in an artistic manner for the *dénouement* which, as all our readers are aware, is the succumbing of the Martians to the bacteria of this earth. This conclusion, both from an artistic and scientific point of view, strikes us as the best thing in the book. In the paragraph in which Mr Wells explains the end of the Martians, he expresses the place and functions of bacteria so admirably that we cannot forbear quoting it: "There were the Martians—dead!—slain by the putrefactive and disease bacteria against which their systems were unprepared; slain as the Red Weed was being slain; slain, after all man's devices had failed, by the humblest things that God, in His wisdom, has put upon this earth.

"For so it had come about, as, indeed, I and many men might have foreseen, had not terror and disaster blinded our minds. These germs of disease have taken toll of humanity since the beginning of things—taken toll of our pre-human ancestors since life began here. But by virtue of this natural selection of our kind, we have developed resisting-power. To no germs do we succumb without a struggle, and to many—those that cause putrefaction in dead

matter, for instance—our living frames are altogether immune. But there are no bacteria in Mars, and directly these invaders arrived, directly they drank and fed, our microscopic allies began to work their overthrow. Already when I watched them, they were irrevocably doomed, dying and rotting even as they went to and fro. It was inevitable. By the toll of a billion deaths man has bought his birthright of the earth, and it is his against all-comers: it would still be his were the Martians ten times as mighty as they are. For neither do men live nor die in vain."

Life without a Stomach

Although the Martians, and perhaps our remote descendants, may be able to live without stomachs, yet the possession of that organ is usually reckoned a necessity for human life at the present day. It is therefore surprising to learn that the entire stomach of a living woman affected by a tumour was removed by Dr Carl Schlatter of Zurich at the beginning of September last, and the woman is still alive and well. Attempts made at St Louis and Milwaukee to repeat the operation on other subjects have not proved successful. At the same time the success of the original operation has shown that the stomach is a less essential organ than is usually imagined. Vomiting took place some time after the removal of the organ, in some instances to a considerable extent, suggesting that the remaining portion of the duodenum was becoming distended as a new receptacle for food. Dr E. C. Wendt, writing in the *Medical Record* (New York, Dec. 25), points out among the conclusions to be drawn from this operation that: (1) The fluids and solids constituting an ordinary mixed diet are capable of complete digestion and assimilation without the aid of the human stomach. (2) A gain in the weight of the body may take place in spite of the total absence of gastric activity. (3) The general health of a person need not immediately deteriorate on account of removal of the stomach. (4) The most important office of the human stomach is to act as a reservoir for the reception, preliminary preparation, and propulsion of food and fluids. It also fulfils a useful purpose in regulating the temperature of swallowed solids and liquids.

A modern Menenius can no longer show the folly of the strike of the members, since the members can readily reply that if the belly continues to give itself airs they will simply chuck it out.

The Ripening of Cheese

In our February number (p. 77) we had some remarks about ferment action which will enable our readers to appreciate the import-

ance of a paper by Messrs S. M. Babcock and H. L. Russell, in the fourteenth *Annual Report* of the Wisconsin Agricultural Experiment Station (Dec. 1897). The ripening of cheese is usually supposed to be due to the action of bacteria present in the milk from which the cheese is made. The green or unripe cheese is hard, elastic, and insoluble, being opaque in thin sections. As it ripens it becomes softer, more soluble and translucent, while its nutritious constituents are rendered more soluble and therefore more readily digestible. The decomposition of the proteids produces in the mature cheese albumens, albumoses, peptones, amido products (tyrosin, leucin) and ammonia. The ascription of these changes to bacteria is supported by the great development of lactic acid bacteria in hard cheeses, while the experimental elimination of those organisms has seemed to confirm the idea that they were closely connected with the ripening. The American authors, however, have been investigating the enzymes or unorganized ferments of milk, and conclude that these alone are sufficient to produce a series of decomposition-changes similar to those found in ripening cheese, even when all bacterial ferments have been carefully excluded. The changes that occur were proved to be of a non-vital character and undoubtedly due to enzymes. These enzymes, however, may themselves have been produced by bacteria originally present in the milk, or they may be inherent in the milk itself. That the latter is the case was shown by securing milk drawn from the cow with great care, and treated immediately with antiseptics; for this freshly secured milk underwent identical changes. It was found possible to separate enzymes having a curdling action on the milk, as well as those with digestive functions. It is therefore the belief of the authors that the ripening of hard cheese under normal conditions is caused by the joint action of organised ferments (bacteria) and unorganised ferments (enzymes). The breaking down of the casein is due rather to the enzymes than to the lactic acid bacteria.

After Mr Wells' magnificent testimonial, quoted above, this is rather a come-down for the bacteria. At the same time, it does not seem probable that the lovers of Gorgonzola, Camembert and Stilton, least of all "Limburger käse," will be anxious to purchase their cheese sterilised; for our authors still consider that in the production of the characteristic flavours bacteria plays a somewhat important part.

Sleep Movements

The sleep movements of the leaves of certain plants noticed by Linnaeus form one of the many subjects of investigation by Charles Darwin, who gave an account of his interesting experiments and observations in his book on the "Movements of Plants." If we com-

pare the day and night position of the leaves of clover, Canadian bean, or *Robinia*, we see that the result, effected by somewhat different movements, is the same, namely, to expose as little as possible horizontal leaf-surface. In the bean and *Robinia* the leaflets hang more or less vertically, in clover they are arranged in a neat little packet. The view taken by Darwin, and generally held since, is that the object of the movement is to prevent an excessive radiation of heat. The leaf is necessarily a delicate structure, functioning as the seat of the most important life-processes of the plant, and it is easy to conceive the danger of damage from excessive loss of heat by radiation to a clear sky if the leaf retained at night its horizontal day position.

In a recent paper (*Botanische Zeitung*, vol. lv., 1897, p. 71), Stahl criticises this view, and suggests that leaf-movements are associated with the giving up of water-vapour (transpiration). The object of transpiration is the maintenance of a current of water carrying in solution food-stuff absorbed from the soil by the root up to the leaves, and the sleep-movement, far from being a sign of repose, is an adaptation for enabling this process to go on by night as well as by day. In most plants the apertures from which the water-vapour escapes, or stomata, are closed all night and transpiration is reduced to a minimum. Stahl finds, however, that in cases where the leaves show the nyctitropic movement the stomata remain open. The object of the vertical position seems to be to prevent deposition of dew on the leaf, and merely to remove an effective obstacle to transpiration of water-vapour. In some plants the night position is also assumed in very strong sunlight. There the action has the opposite effect, namely, to check transpiration and remove the danger arising from excessive loss of moisture. It recalls the permanent vertical position of the leaves of many Australian Mimosas, Eucalyptus and Hakeas, which are exposed to intense sunlight. Stahl suggests that the spontaneous up and down movements of the leaflets of the American telegraph-plant (*Desmodium gyrans*), are to promote transpiration, the motion causing a continual removal of the saturated air near the leaf, and the shivering of the leaves of the aspen and many poplars may have the same meaning.

Turgidity

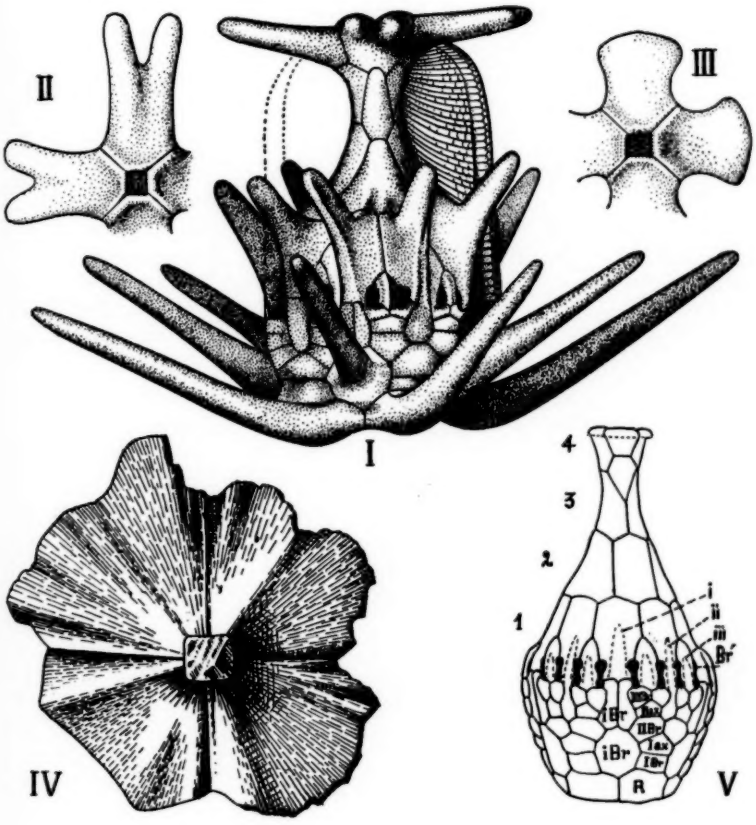
In connection with the amount of water in plants, the experiments of Mr E. B. Copland on "the relation of nutrient salts to turgor," described in the December number of the *Botanical Gazette*, are of interest. Since the work of Pfeffer and others on the hydrostatics of the plant-cell, and the demonstration of the great internal pressure set up by osmosis, the turgidity of the cell has been an important factor in the explanation of plant-growth and movement. As in the case of other newly-discovered causes, it was at first made responsible for too

much, and views of the relation between turgor and growth have materially changed within the last ten years. The experiments now before us were undertaken to ascertain the influence of various chemical elements on the turgor of the plant-cell. The results were however almost entirely negative. Potassium alone showed any direct influence, its removal as a food-substance being followed by a diminution of turgidity.

Potassium presented in solution to the roots of plants causes the cells of both root and stem to show a higher turgor than they do when it is replaced by sodium. From the analysis of the sap, the writer concludes that the influence is a direct one. When offered to the root, the potassium salt is taken up and stored in the cell-sap, where it becomes an important part of the osmotically active material which keeps the cell and plant turgid. It is difficult to understand this decided insistence of the plant for potassium, and the uselessness of the far more plentiful and physically lighter sodium.

The Riddle of Cryptodiscus

In 1864 Professor James Hall figured a peculiar fossil from the Niagara Limestone, apparently of Wisconsin, and gave it the name *Cryptodiscus* (Fig. IV). No notice was taken of this, and it was not so much as mentioned by Mr S. A. Miller in the various editions of his catalogue of North American fossils. Mr Miller himself, however, in 1892 figured a portion of a similar fossil from the Niagara Limestone of Indiana, indicating it as "? ? ? ? ? ? ?." In the *Journal of Geology* (Vol. V., No. vii., pp. 744-751), published at the close of 1897, Mr Stuart Weller, of the University of Chicago, describes and figures four different forms of the so-called *Cryptodiscus*, from the dolomitic Niagara Limestone of Joliet, Ill., and applies to them as many specific names. No diagnosis of *Cryptodiscus* is given, so that it is a little out of place for Mr Weller to say that Hall's name was "never properly published." The fossil may, however, be described as composed of four plates, each roughly forming the quadrant of a circle, opposed by their straight edges, meeting around the centre in a downward prolongation of square or sub-circular section, in which a central canal is left, and often forked or extended in several branches on their outer margins. Hall regarded *Cryptodiscus* as "the calyx of a Crinoidean? of a new and peculiar type"; but Mr Weller's interesting article is largely devoted, as its title indicates, to proving that the fossils represent "the casts of the gastric cavities of medusae." We are spared the trouble of criticising this not very plausible suggestion, through a footnote added by Mr Weller on sending the paper to press. Specimens in the collection of Mr E. E. Teller, of Milwaukee, "seem to establish the fact that *Cryptodiscus*



A FOSSIL CRINOID

Fig. I.—Restoration of *Callicrinus murchisonianus*, after Angelin ; an arm remains on the right, the others having been removed. Fig. II.—The circlet of plates around the arms, seen from above. Fig. III.—The same circlet in *C. costatus*, after Angelin. Fig. IV.—"*Cryptodiscus*," after Hall's original figure. Fig. V.—*Callicrinus costatus*, with the arms removed ; Br', the place of attachment of the arms ; i, ii, iii, processes, some of which form the middle circlet of large spines in *C. murchisonianus*. The drawings are by Mr G. C. Chubb, and are larger than natural size.

is a remarkable disk-like expansion of the four plates forming the terminal ring of the anal tube of some crinoid, probably *Callicrinus*." We have no doubt whatever that this is the correct interpretation. *Callicrinus* (Pl. IX., fig. V.) is a genus that does not differ in essentials from *Eucalyptocrinus*, and its presence in America has only recently been established by Wachsmuth and Springer. Its remains occur in the Niagara Limestone, associated with *Cryptodiscus*, and we would specially point to *C. cornutus*, Hall sp., as likely to be connected with "*Cryptodiscus*." At any rate, no one acquainted with the *Callicrini* of Europe and their strange anal expansions can doubt that *Cryptodiscus* represents the same structure; we may refer Mr Weller to Angelin's "Iconographia Crinoideorum" (Stockholm, 1878), plate xxiv., figs. 25, 26, *Callicrinus costatus*; plate xxviii., figs. 14, 15, 16, *C. murchisonianus*; and fig. 18, *C. koninckianus* (Figs. I., II., III.).

Since the above was written, the December number of the *Journal of Geology* has come to hand, and in it is Mr Weller's fuller description of Mr Teller's specimens, this time as *Callicrinus*. There is, however, nothing of importance to add to what has just been said. Mr Weller may be congratulated on the promptness and frankness with which he repairs an error, but it is a pity that his editor could not manage to eliminate his first paper; for, though the value of his figures and careful descriptions is undiminished, the specific names must eventually prove synonyms of those already given to the calyces. Our American friends like to be in a hurry, and Mr Weller has not even taken time to give either the measurements of all his species or a statement of the relative size of his figures, or the dates and page-numbers of the passages he quotes. Nor can one tell from his figures whether the objects are concave or convex. There is at this moment a wide opening in America for a new student of fossil echinoderms, but unless one appears who will turn out far more careful work than has hitherto been published, we shall not welcome him with any rapture.

Pettifogging Science

When we spoke of "our American friends," we did not mean to imply that there were no such offenders in other parts of the world, nor did we forget that America too has its earnest workers. We are urged to this explanation by an admirable address from a leading American biologist, published in *Science* for January 14. Dr C. O. Whitman, who, it is interesting to note, is connected with the same University as Mr Stuart Weller, writes as follows: "The avalanche of modern biological literature consists too largely of scrappy, fragmentary, disconnected products of a multitude of investigators, all working as so many independent individuals, each snatching what-

ever and wherever he can, and then dumping his heterogeneous contributions into the common hodge-podge. How are we ever to extricate ourselves from such appalling confusion? The ambition to be prolific rather than sound is a peril against which we seem to have no protection at present. And yet, if I mistake not, there is a growing sentiment against such traffic in science, which will eventually make it plain that ambition in that direction spends itself in vain. A dozen or more dumps a year, with as many or more retractions, corrections and supplements, is only a modest-sized ambition. Conclusions are palmed upon the unsuspecting reader, and then, without compunction or apology, reversed from day to day or from month to month, or, worse still, in an appendix subjoined, so that it may be seen how little it costs to be prolific when one day's work cancels another."

Dr Whitman casts about for a remedy. He sees that every worker cannot have Darwin's industry and reserve, but he thinks that something might be done by training students in laboratories to work on definite problems in co-ordinate groups, "each performing his mite in conjunction and relation with the others of his group." Such a method of work would, he points out, be of advantage not only to science but to the workers themselves, for instead of working in a cloistral and jealous seclusion, they would be brought into active and mutually helpful relations, and enabled to draw from one another the best that each could give. This happy result is to be brought about in great measure by combining instruction with investigation in the various scientific laboratories. We grant that teaching is as valuable to the teacher as to the taught, but we are a little doubtful whether this introduction of socialism into scientific investigation would not tend to check individual enterprise and to slacken actual advance by taking away the spur of competition. The remedy, it seems to us, is to encourage so far as possible the idea that one solid piece of work is worth more than a dozen driblets, to look with suspicion on the prolific pamphleteer, and to continue to cast ridicule on that worse than ridiculous nuisance—the preliminary noticer. Let us insist upon exactness of description, accuracy of drawing, correctness of reference, lucidity of style, in short, upon all those qualities that go to stamp a scientific monograph as classical.

The Placentation of Perameles

The full account of Mr Hill's discovery of a true allantoic placenta in the marsupials *Perameles obesula* and *nasuta*, published in the *Quart. Journ. Micro. Science* (vol. xl., pp. 385-446, plates 29-33), is one of the most important of recent contributions to zoology. The

facts are startling in themselves; they are described luminously, illustrated by beautiful figures, and they raise a number of novel questions. Mr Hill has been able to collect material consisting of six stages—some from *Perameles obesula*, some from *P. nasuta*, but the processes in the two cases he declares to be so similar that he was able to take them consecutively without reference to specific difference. The uterine wall prepares itself for the attachment of the embryo in a fashion unknown in other mammals. In the normal way, the mucosa hypertrophies; the uterine glands enlarge; the interglandular connective tissue forms a loose network of cells permeated by lymph and the blood-vessels enlarge. But the superficial epithelium loses the outlines of its cells and becomes a syncytium, which increases in thickness by the multiplication of nuclei and protoplasm, and the blood capillaries penetrate the syncytial protoplasm until they form a network upon and just under the surface.

The embryo attaches itself to this syncytium by means of enlarged ectoderm cells over the discoidal area of true chorion, with which the allantois fuses. These ectoderm cells become resorbed, and the allantoic capillaries grow down deeply into the syncytium forming interdigitations with the maternal capillaries, the two sets of vessels being separated only by their endothelial walls and a little syncytial protoplasm. Thus a true, discoidal, allantoic placenta is formed. It is non-deciduate; at birth not only is there no loss of maternal tissue but an area of the allantois remains attached to the syncytium, and gets absorbed by maternal leucocytes. In addition to the allantoic placenta, there is a temporary yolk-sack placenta, somewhat annular in character, and functional before the allantoic placenta is ready.

Mr Hill discusses the bearing of his discovery on the relations between marsupials and Eutheria. He rejects the supposition that the allantoic placenta of Perameles is a convergent but independent structure. Like Semon and Hubrecht, he lays weight on the functionless degeneracy of the allantois in marsupials generally, and on the degenerate character of the milk dentition of marsupials. He inclines to the belief that Eutheria and Metatheria are parallel branches from a common stock, which possessed a discoid, allantoic placenta.

A Fresh Mammoth

We learn from the *Révue Scientifique* that Mr K. Nossilov, editor of the *Novoye Vremya*, has discovered an entire mammoth in Ia-Mala Peninsula in the country of the Samoyedes. In May 1897, he arrived at the mouth of the river Jouribey, and learned that two years previously the body of a mammoth had been found by the

inhabitants exposed on one of the banks of the river. It still retained its skin and wool, and even the tusks, for they were so firmly fixed that the Samoyedes had been unable to withdraw them. Mr Nossilov did not himself remove the mammoth, but he suggests that here is an excellent opportunity for some museum or university of his country to procure a mammoth in a complete state of preservation. Even the famous mammoth skeleton in the Academy of Sciences at St Petersburg is very far from perfect. Many parts are restored in wood, and the tusks do not belong to the same animal as the other portion of the skeleton.

Geological Photographs

The Eighth Report of the British Association Committeee for the collection of photographs of geological interest in the United Kingdom, just issued, details those which had been received by the committee during 1896-97. The collection, which is stored at 28 Jermyn Street, and which is accessible to students on application, now numbers 1751 items. No less than 364 new pictures were received during the year, among them being a series from the Wealden area, from Nottingham, North Staffordshire, the Sgurr of Eigg, Yorkshire Caves, County Dublin, Yorkshire, the Isle of Man, Devonshire, Isle of Wight, Charnwood, Yorkshire Dales, and North Wales. The most valuable series came from that exquisite artist, Mr R. Welch, of Belfast, who contributed no less than 100 platinotypes of wonderful excellence of Irish geology. It is difficult to over-estimate the value and utility of such a collection, and the committee state that several of the features preserved have now disappeared. We would especially urge all those who follow geological nature with a camera to assist this committee by forwarding platinotypes unmounted to the secretary, Mr W. W. Watts, of the Mason College, Birmingham, and it is to be remembered that a photograph showing geological features, even if not specially taken from a geological point of view, may be of great value as a record for geologists and others.

Arrangement of Chambers in Foraminifera

A possible explanation of the quinqueloculine arrangement of the chambers in the young of the microspheric forms of *Triloculina* and *Biloculina* is offered by Mr J. J. Lister in a paper recently read before the Cambridge Philosophical Society. In the megalospheric forms of these genera the arrangement is very simple, is followed from the beginning, and is disposed on either side of an axis, the median plane which divides any single chamber symmetrically being

the median plane of the whole series of chambers. In the microspheric form, as shown by Mr Schlumberger, the arrangement at first is that which characterises the genera *Quinqueloculina* during its whole growth. In the young stage of the *Biloculina* and *Triloculina* the plane dividing any chamber symmetrically is not identical with the corresponding plane of the preceding chamber, but directed at a definite angle to it. Mr Lister thinks that the megalospheric form is produced asexually, and has a direct development, while the microspheric form is produced sexually, and during its growth repeats the arrangement characteristic of allied forms before it attains the arrangement proper to its own genus. This is made much clearer by Mr Lister in his paper with the accompanying diagrams, and is on a par with the life history of *Clavellina* among the ascidians.

Missing Word Problem in "The Annals"

We announced some months ago that the trustees of the British Museum (Natural History) had commissioned one of the staff to prepare a catalogue of moths. As this is to be a standard book of reference for students and the public, we venture to hope that the quotations of previous authors may be a little more explicit than those in the twenty-sixth article of the February number of the *Annals Mag. Nat. Hist.* Entomologists may be quite familiar with their vast literature, but the unfortunate student who is endeavouring to acquire some knowledge of his subject may rack his brains over such quotations as :—

Wlk., xxx. 955.
Koll. Hüg. Kasch, iv. p. 494.
Rag. Nouv. Gen., p. 44.
Feld. Reis. Nov., pl. cxxxvi., fig. 40.

One would also like to see uniformity in the abbreviation of author's names. Swinh., Schiff., Butl., and Fabr. convey some meaning, but one cannot say the same of Wlk., Wllgrn., Hmpsn.

Forthcoming Congresses

The International Congress of Physiologists is to be held in Cambridge during the week beginning 23rd August, that is to say, contemporaneously with the Zoological Congress. We are very glad to learn that the Executive Committee of the latter Congress has been taking more active steps than at first seemed probable to obtain the co-operation of all British zoologists, in which name we include not only Irish and Scottish, but those in India and the Colonies. To enable this country to keep up the standard of hospitality that has hitherto been maintained at this Congress, subscriptions are desired

and may be sent to the treasurers, Dr. P. L. Sclater and Professor S. J. Hickson at 3 Hanover Square, London, W. The Zoological Society of London has itself contributed £100. In connection with this Congress, a very successful meeting of representatives from natural history and scientific societies in the north of England was held in Manchester on the 16th of February.

The British Association will meet in Bristol during the week beginning 7th September, under the presidency of Sir William Crookes. The local secretaries, Mr Arthur Lee and Dr Bertram Rogers, have already made several arrangements. The reception rooms are to be in the Victoria Rooms, Clifton; the public lectures will be given in the large Colston Hall; while the sections will meet in rooms provided by the Museum Committee, University College, the Society of Merchant Venturers, the Charity Trustees, the Bishop of Clifton, and others. There will be a picture exhibition, with a military band, in the Drill Hall. More scientific in its interest is a biological exhibit being organised by Mr E. J. Lowe, Dr Harrison, and Prof. Lloyd Morgan at the Zoological Gardens, Clifton. The excursions promise to be exceedingly interesting, since they will include Aust Cliff with its well-known section of Lias, Rhaetic, and Trias; Tortworth with its Silurian beds; Stanton Drew, with its megalithic remains; the Cheddar Cliffs and Caves, the sources of the Bristol water supply; the Severn Tunnel, Cadbury Camp, Swindon, Avonmouth, Wells, and Glastonbury, where the Mayor of Wells, the Dean and Chapter, and residents will entertain the visitors to lunch, and the Mayor of Glastonbury will provide tea in the old Abbot's Kitchen. Other excursions to Salisbury and Stonehenge, Nailsworth and Stroud, Longleat and Raglan Castle are under discussion.

The ninth International Congress of Hygiene and Demography will be held at Madrid from April 10th to 19th. The sections under which subjects will be discussed are as follows: Microbiology in Relation to Hygiene; Prophylaxis and Transmissible Disease; Medical Climatology and Topography; Urban Hygiene; Hygiene of Alimentation; Hygiene of Infancy and of Schools; Hygiene of Exercise and Labour; Military and Naval Hygiene; Veterinary Hygiene, Civil and Military; Sanitary Architecture and Engineering; Technics of Demographic Statistics; Statistical Results in Relation to Demography; Dynamical Demography (movements of population, etc.). The secretary is Dr Cabañas, the University, Madrid.

I

A New Scheme of Geological Arrangement and Nomenclature

Part III

I NOW propose to come to closer quarters with my subject, and to do so by calling in question Lyell's scheme of arrangement of the Tertiary beds, which still largely holds the field, as unscientific, mischievous and misleading.

I have not been able to find who first used the term Tertiary in its modern sense—namely, as equivalent to the greater part of the beds above the chalk—nor when the term was first used. Perhaps some reader of *Natural Science* could help me.

Cuvier and Brongniart in their famous memoir on the Paris basin; Webster in his memoirs on the beds of the Isle of Wight and the Thames basin; and Bonelli, Brocchi and other Italians in North Italy and Sicily, had explored with skill and acumen the Tertiary deposits in the localities referred to; but none of them had attempted any general scheme of general classification of the Tertiary beds as a whole. This was the work of the conchologist Deshayes, who, on the evidence of the fossil shells they contain, roughly arranged the beds above the chalk (excluding the beds being currently deposited) into three groups. Lyell, after his famous visit to Italy and France, brought back with him Deshayes' arrangement, and gave names to the latter's anonymous divisions. These names, he tells us, he adopted in consultation with Dr Whewell. Thus arose the terms Eocene, Miocene and Pliocene. These three divisions included everything which Lyell deemed to be Tertiary.

Above the Tertiary beds he placed the deposits which are now being made by river and sea and volcano, etc.—*i.e.*, the actually current beds we see accumulating everywhere now. These beds he called *Recent*, and it is important to remember that by 'Recent' Lyell distinctly meant Non-Tertiary. To him they belonged to an entirely different order of things to anything in the Tertiary beds, and they answered in fact to the beginning of another cycle of Geological history, such as we mark the importance of when we speak

of the Primary, Secondary and Tertiary periods. This separation of the Recent beds was widely, almost universally, accepted; and presently—namely, in 1849—a new name was given to them by D'Archiac, namely, *Quaternary*, thus emphasizing the enormous importance which they held in the eyes of a popular school of Geology, since the very name Quaternary was merely the completion of the cycle of names already referred to—namely, Primary, Secondary and Tertiary; and this term Quaternary not only still lives, but is in continuous use, and especially by the geologists of the continent.

Now, some of us know well, or think we know, the vast difference that separates the Primary, Secondary and Tertiary beds. The huge gap which separates their fossil contents in our latitudes makes it necessary to deal with them in any systematic scheme of geology as ultimate and supreme factors.

In order to create a fourth division co-ordinate with them, we ought assuredly to have co-ordinate conditions separating the Tertiary beds from the 'Recent' beds of Lyell and the Quaternary of other writers. Let us turn then to Lyell's own words, and see how he defined his Recent beds, and by what *criteria* he separated them from the beds he called Tertiary.

I shall quote his own definition: "All sedimentary deposits, all volcanic rocks—in a word, every geological monument, whether belonging to the animate or inanimate world, which appertains to this epoch, may be termed recent. Some recent species, therefore, are found fossil in various Tertiary beds; and, on the other hand, others, like the Dodo, may be extinct, for it is sufficient that they should once have co-existed with man, to make them referable to this era." Again he says: "We may sometimes prove that certain strata belong to the recent period by aid of historical evidence, as parts of the delta of the Po, Rhone and Nile, for example; at other times, by discovering imbedded remains of man or his works; but when we have no evidence of this kind, and we hesitate to ascribe a particular deposit to the recent era, or that immediately preceding, we must generally incline to refer it to the latter, for it will appear in the sequel that the changes of the historical era are quite insignificant when contrasted with those of the newest Tertiary period."

Here then we have the ear-mark by which Lyell discriminated his Recent or Post Tertiary beds, namely, the presence in them of man and his works, and that alone. This may have been pardonable in 1830, but what is to be said of it now? What is to be said for a classification which is based professedly on biological evidence, and which treats the introduction not of a very specialised mammal or series of mammals differing essentially from their predecessors? but

of one belonging to a large class of mammals, the species Bimanus, and that species alone, as the foundation, not of a new subsection of a particular series of beds, but as equivalent to the portentous changes involved when we compare the beds of the so-called Primary age with those of the Secondary, and those of the Secondary with those of the Tertiary. The position is too ridiculous for argument. Such a classification is an inversion of all logical system and method. It has frequently been protested against, and cannot, it seems to me, be justified in any way whatever. Far from being separable from the Tertiary series, the beds called Recent by Lyell, and Quaternary and Post Tertiary by others, ought to form in essence a very small and very intimate section of the beds of the Tertiary period itself. If the term Tertiary is to be logically used, it must include ourselves and our works. We are not living in Post Tertiary times; we are living in Tertiary times, and we shall continue so to live until a much mightier change has occurred in this world than the addition in a common cemetery of the bones of Shakespeare and Bill Sykes to those of many a buried Yorick, who once cracked nuts when hanging from a shady branch by his tail.

Let us proceed, however. This is not the only objection to Lyell's classification. When Lyell first wrote his Principles, the incoming of Man was supposed to be a comparatively very recent event. The fact that he had lived with such extinct beasts as the Mammoth in Europasia, the Mastodon and the Megatherium in America, was not generally suspected. It required a great many years of patient pleading and hammering by unknown but careful observers like Schmerling in Belgium, and MacEnery in Devonshire, and by others, to whom the world was deaf, before the fact was accepted by the Papal authorities of latter-day science. Be it remembered that no one was more to blame for this neglect and rejection of the facts and their teaching, for more than one decade, than Huxley himself when secretary of the Royal Society, and let us beware of surrendering ourselves to the science of "the Syllabus" in consequence.

We now know that not only was man contemporary with the extinct animals just referred to, but that in all probability he lived with the great Southern Elephant, *E. meridionalis* in Italy, and was also contemporary with the same animal in England, as attested by recent evidence from the forest bed in Norfolk. Inasmuch as (so far as we can tell) the forest bed in Norfolk was contemporary with the latest of the three divisions of the marine crag, the so-called Norfolk or Weybourne crag, it follows that Lyell's name "Recent beds," as defined by himself, and the names subsequently substituted for that phrase, are really equivalent to a great deal more than he supposed, and make a great invasion into his Pliocene horizon.

The names in question, therefore, *i.e.*, Post Tertiary, Quaternary, etc., have a double frailty. Need I press my case in this behalf further. I trow not, but, you will say, how can I presume to write in this way when I have been guilty of using this stupid nomenclature myself. Of course I have been guilty of it. I have nevertheless been ashamed of myself and am so still. My only excuse has been that in order to be understood we must use the common shibboleths of conversation, and in science these shibboleths are the technical phrases of the text books. It does not make the boat less leaky and unseaworthy because I happen to be in it along with the great guns of geology, but I do try to make amends by professing my discontent and shame. I challenge the authors of several recent text books of repute to do the same. Let them justify, if they can, the further use of such terms as Quaternary, Post Tertiary, or Recent (in the sense and with the connotation that Lyell used the word recent). If they cannot justify them let us all agree to disuse them as misleading and ridiculous and take a common stand on the platform that we are all Tertiary Beasts, just as much as that stiff-necked brute the Titanotherium, and that the Tertiary period comes right up to to-day.

Let us now turn to Lyell's division and classification of the Tertiary period. Lyell not only took over Deshayes' main divisions of the Tertiary beds, but he also took over the former's general criterion by which they were to be discriminated. This in essence did not depend upon stratigraphical considerations or upon natural breaks in the sequence, either by unconformability or otherwise, such as we accept as our criteria in the older rocks, but upon entirely different considerations, namely, the numerical proportion of extinct to living forms in any particular bed.

It must be also remembered that in Deshayes' scheme and also in Lyell's the classification proposed was entirely based on the proportion of extinct to living *marine testacea* only, and had application therefore to marine beds only and had no reference of any kind to the fauna of the land.

Deshayes' method was called in question when it was first published both in England and in France, *inter alia*, by Bakewell and Desnoyers, as being based on utterly arbitrary, uncertain, and shifting data. Who is to know until the ocean bed has been scoured from end to end what species are still living? Every fresh dredging expedition on a considerable scale adds to the number of molluscs once supposed to be extinct and which are found to be still alive, many of them (as in fact many of the fossil molluscs also) having very local and limited distribution.

The problem was further sophisticated by the fact that some of the beds point to conditions of deposition similar to those in the

neighbouring seas; while others point to tropical or semi-tropical conditions now only existing in latitudes where the dredge has only been applied in most perfunctory ways. The method employed is also largely dependent on the personal equation of observers who, unfortunately, have been separated by our ridiculous theories into separate schools of students, one school dealing with recent and another with fossil molluscs, as if either could possibly be studied satisfactorily without continual reference to the other. The consequence has been the creation of a number of fossil species which had already been described and named as recent. Lyell was himself aware of some of his own difficulties. Thus, in 1836, he calls attention to the discrepancy in the results obtained by Deshayes and Dr Beck, when they examined the proportion of living to extinct forms among the crag shells, Dr Beck going the length of saying that, although a larger proportion of shells in the crag approach very near to others which now live in our northern seas, he regarded them as almost all of distinct species and unknown as living. Lyell adds the pertinent sentence. "In regard to the discordance in the results at which these eminent conchologists have arrived, it may arise, not only from the unequal opportunities which they have enjoyed of examining the necessary data, but also in part to the different estimate which they have formed of the amount of variation necessary to constitute a distinct species. One instance," he says, "will sufficiently illustrate my meaning. Those naturalists who agree with M. Deshayes in referring all the living varieties of *Lucina divaricata* brought from different countries to one and the same species, will identify many more fossils with recent shells than those who agree with Dr Beck in dividing the same recent individuals of *Lucina divaricata* into six or eight distinct species" (*Proc. Geol. Soc.* ii. 372-3). Again, according to our modern lights, the proportion of extinct to recent forms in a particular place may be largely accidental, a shift of the sea bottom might divert the Gulf Stream, or the corresponding cold Newfoundland current, and utterly change the molluscan fauna of a particular district, and yet be only a local and adventitious fact.

The only virtue of the criterion in question, if it has one at all, is that it is of universal and not of mere local application. We can, no doubt, count the shells in a New Zealand bed as we can in a crag bed, and we can then compare the proportions of living to extinct forms in them both, and thus establish a common measure, but the common measure is an utterly misleading and absurd one. The geological history and conditions of the two life-provinces are so different that no rational conclusion can be derived from the fact that two beds at the antipodes to each other happen at a particular moment to contain the same number of recent forms. The recent

forms themselves may be, as in the Australian seas, very old and primitive types, while in our seas they are very young and specialised types. The *Lingula*, which has been extinct in European seas since remote geological time, still lives in the Australian seas. The conservation of the biological record is as remarkable in some latitudes as its tendency to radical change is in others, and we are utterly deluding the student with false notions of contemporaneity of deposition, of homotaxis or other similar homologies when we apply the same name to two widely separated beds, not because they contain the same or a similar fauna, but because the proportion of living to extinct forms is the same, or nearly the same, in each. We do not put negroes and Europeans in the same category because in some cases there is the same proportion of rogues to honest men among them, or because of any other purely adventitious circumstance.

A truly scientific classification of the Tertiary beds must, if it is to have any permanent value, be based on more stable data than the proportionate numbers of living forms to extinct ones which occur in a particular bed. The futile character of this criterion applies to it under all circumstances and in all kinds of beds, subaerial or otherwise.

In the case of the Tertiary beds, however, the classification in question became more especially inconvenient and misleading since, without any explanation or justification and in a measure surreptitiously, a mode of classifying beds dependent experimentally upon the proportion of the mollusca in the marine beds was transferred to a series of subaerial beds, where the same criterion was never in fact used as a touchstone. This was done apparently on the very superficial ground that De Beaumont had divided the extinct mammals into three series, respectively marked by the palaeotheria, the mastodons and elephants, and this threefold division of the mammalian fauna of the Tertiary beds was adopted and incorporated by Lyell into his scheme for arranging the marine beds without any inquiry as to whether the divisions in the one case corresponded with the divisions in the other, or could consistently be made to so apply. Names originally based on facts derived from the molluscs were quite arbitrarily applied to facts derived from land animals. This is a very good instance of what I have complained of, namely, the condensing of submarine and subaerial beds into a common series with common names.

On these and other grounds I venture to distrust and rebel against Deshayes' and Lyell's criterion for distinguishing the various divisions of the Tertiary period and the conclusions based on them.

Let us now advance again and approach more concrete ground.

The most recent of Deshayes' divisions was that to which Lyell gave the name of Pliocene. Like the other names he gave to the principal divisions of the Tertiary period, it was, as he tells us, a joint composition of his own and Whewell's, and in this case was a derivative from πλειων major and καινος recens, and, as Lyell says, was meant to exclude all beds in which the majority of the species of testacea belong to living forms, and at the same time, as we have seen, it excluded all beds in which traces of man had been discovered. It was also co-extensive in its connotation with the latest of the three divisions of Deshayes'.

Now Deshayes' discrimination of his third or latest series of beds was entirely based upon the Italian beds, the beds of the Subapennines, as he first called them, and those of Sicily and the south of Italy respectively. So long as we restrict his definition to that area I see no great cause to quarrel with it.

Lyell's particular part in the business seems to me to be distinctly illogical and mischievous. In the first place he included within the definition of the series and within the connotation of the term Pliocene the English crag beds.

Now I venture to think, and I want to speak very emphatically upon this point, any scheme which includes the more recent beds in the Mediterranean area and those in the North Sea area in a common nomenclature must be of doubtful value. The recent geological history of those two areas is quite different. Their molluscs are different, and the mere fact that they may have been contemporary no more justifies our describing them under a common name than we should be justified in treating Brazil and Birmingham as parts of one zoological province because the parrots in the one country and the politicians in the other are contemporaries, or in speaking of the era of Shakespeare when describing the history of China at the beginning of the seventeenth century. If it be right to speak of the Sicilian and Subapennine beds and their contents as Pliocene, it seems to me to be utterly wrong and misleading to apply the same name to any beds whatever in the area of the North Sea, and *vice-versâ*.

This is not all. By "the crag was understood in 1830 all the shell-bearing beds of eastern England, from the base of the Coralline Crag upwards, including all the drift beds containing marine shells. Inasmuch as Lyell excluded from his Pliocene horizon all deposits contemporary with man, it is clear that he was mistaken in one respect or the other, for, as we now know, the upper crag in the sense in which he used the word was contemporary with the mammoth, which was contemporary with man. By using the term Pliocene, therefore, in the sense in which the inventor used it, we are landed in a complete quagmire. In that quagmire it seems to

me every subsequent inquirer into this particular corner of the geological field has fallen. The only escape from it is either to drop the term as applied to the English beds altogether, and to let it stand (if thought right) for the Mediterranean beds alone, or to try in some way to entirely alter the definition and connotation of the term from what it originally meant, and from what its author and inventor meant. This seems very much to me like referring to some dull, sober, quiet, orthodox geologist by the name of Howorth. I cannot put the *reductio ad absurdum* more thoroughly.

Let us, however, advance again. Lyell in taking over Deshayes' division of the Tertiary beds altered it in one respect. He divided it into four series instead of three, and in my view altered the French writer's classification very much for the worse. He altered it by, in fact, separating the uppermost division which had been discriminated by Deshayes into two divisions, to which he gave the name of older and newer Pliocene respectively. This would have been acceptable and useful if applied to the Mediterranean series of Deshayes alone, and if it had been understood that it merely meant the breaking up of one of Deshayes' main divisions into two subordinate ones, but this is not at all what Lyell meant. In his scheme each of the two divisions of the Pliocene series is itself given co-ordinate rank with the other two great divisions of the Tertiary beds, namely, the Eocene and Miocene (see his table *Principles of Geology*, ed. ii., vol. iii., p. 61), and he emphasised this very strongly in the French translation of the *Principles* by introducing for the first time the term Pleistocene, which he made the equivalent of his "later Pliocene." Subsequently he dropped the two divisions of the Pliocene and divided the whole Tertiary series into four great sections, namely, the Eocene, Miocene, Pliocene, and Pleistocene.

Here again, it seems to me, we have another instance of an entire absence of proportion and of perspective in Lyell's method of classification.

The differences which separate Eocene from Miocene life are assuredly patent enough, and they involve real and substantial biological and other differences. The same is the case when we distinguish between the beds classed as Miocene and those classed as Pliocene; but what sort of differences at all comparable to these separate the Pliocene from the Pleistocene. Even if we limit ourselves to Italy and its marine beds, they are so slight that Deshayes condensed them both into one series; but as applied elsewhere by Lyell, namely, to the countries of the North Sea, they are, with our present knowledge, positively ridiculous. They were always quite inadequate, however, and at the very most justified the creating of subsections of the Pliocene just as we now have subsections of the Miocene, but nothing more, and to introduce a new term like

pleistocene, with the particular intention of treating it as equivalent in weight and value to the other main divisions of the Tertiary series was intolerable, and would not, in fact, have been tolerated but for the glamour attaching to Lyell's name and the tyrannical influence which his reputation has exercised upon subsequent writers. Here again it seems to me we ought to eject the term pleistocene from our nomenclature altogether, unless we are to perform the indecent part of making it mean and include quite a different thing altogether from that which its original godfather meant it to include.

I spoke of this latter part of Lyell's classification and nomenclature as being ridiculous with our present lights. What I mean is this. When Lyell published his work in 1832 the term Crag was used as a common name for all the shelly, sandy. and gravelly surface beds of eastern England, from the base of the Coralline Crag upwards, inclusive of the so-called middle sands containing fossil molluscs. When the contents of all these beds were mixed together and treated as in fact one common fauna, chiefly noteworthy for the great number of tropical, sub-tropical, and of extinct forms which characterise the Coralline Crag, it was not unnatural that Lyell using his criterion should have put the Crag into his older Pliocene, and that he should have completely excluded Britain from the areas where his Newer Pliocene or Pleistocene were known to occur. Into that upper division, however, he put the well-known beds at Uddevalla. This exclusion of Britain from Lyell's original Pleistocene localities must have a strange look to those who know how frequently the term Pleistocene now occurs in more recent English geological literature as denoting a large series of English beds.

As originally defined, the term Pleistocene included among marine beds only the strata of Val de Noto in Sicily, those in the district about Naples, and in Calabria, some doubtful beds at Morca, and the well-known beds at Uddevalla, while the whole of the Crag was put into the older division of the Pliocene, namely, the Pliocene proper, together with the subapennine beds.

Let us again go on, however. The first person to properly discriminate the different elements in the English Crag was Charlesworth, one of those not too lucky men upon whom the sunlight did not always fall, and to whom those of us who care for these things ought to be grateful indeed, for he did more to elucidate the later English Tertiaries than anybody else. His teaching on this matter was sound. May we hope that he now has peace. He showed very plainly that the Crag consists of three distinct horizons, marked by very different life conditions and contents—namely, the White or Coralline Crag, the Red or Suffolk Crag, and the Mammaliferous or Norwich Crag.

Charlesworth's famous memoir on the Crag was published in the London and Edinburgh *Phil. Mag.*, vol. xxxviii. p. 81, August 1835.

The results obtained by Charlesworth were soon after examined by Lyell, who in a paper read before the Geological Society in 1839, accepted his views, and proceeded to apply his own numerical and proportionate method to them. This led him to perform curious somersaults in nomenclature. As we have seen in his first arrangement of the Tertiaries, he put the Crag into his "Older Pliocene," leaving the "Newer Pliocene" without an English representative.

He now entirely altered this view in a most revolutionary manner, and while he retained the Norwich Crag in his older Pliocene, he transferred the Red Crag and the Coralline Crag to "different parts of the Miocene," while he put the testacea of the fresh water or fluviatile deposits of Cromer and Mundesley in Norfolk, Sutton, Grays, Ilford, &c., into his Newer Pliocene (*Proc. Geol. Soc.*, vol. iii. pp. 129 and 130). In this paper Lyell co-ordinates the older crags of East Anglia with the Faluns of Touraine as Desnoyer had done. This he did again on what I deem the same irrational ground, that the proportion of extinct to recent forms in both was pretty nearly the same. How false and misleading such a comparison must be, is shown by the fact that Searles Wood only found 10 per cent. of the shells common to the Crag and the Faluns, thus proving that the two sets of beds really belong to two entirely different geological provinces, and ought to be classified under separate names.

Again, as we have seen, the shells in the diluvium or drift were originally treated as part of the Crag, a view to which some of us have returned. In November 1836, Mr James Smith of Jordanhill, another of the excellent pioneers of geology, first definitely separated the shell-bearing drifts of Scotland and placed them among the 'Newer Pliocene' (*Proc. Geol. Soc.*, vol. ii. p. 428). In February of the following year Mr Clarke similarly separated the diluvium from the Crag of East Anglia.

In November 1839, Mr James Smith went further and separated the true shelly drift of Scotland, called glacial by some, and to which he now gave the name *pleistocene* (this being apparently the first time the name is used in England), from the submerged forests and sandbeaches there which he calls post-Tertiary (Ed. iii., 149 and 150).

In their paper on the geology of Russia by Murchison and Verneuil in 1841, they applied the same name *post-pleiocene* or *pleistocene* to the so-called diluvium or drift, inclusive of the marine surface shell-beds of Northern Russia, whose contents were correlated with

the beds at Uddevalla, which Mr James Smith had affirmed to be identical with those in the older shelly drift beds round Scotland.

In 1846 Edward Forbes published his well-known memoir on the geological relations of the fauna and flora of the British Isles, and he also used the term pleistocene. "But," says Lyell, "he applied the term almost precisely in the sense in which I have hitherto used Post-Pliocene, and not as short for Newer Pliocene." "In order, therefore, to prevent confusion, I thought it best entirely to abstain from the use of Pleistocene in future; but in a note to my 'Elements of Geology' I advised such geologists as wished to retain Pleistocene to use it as strictly synonymous with Post-Pliocene" (*Antiquity of Man*, pp. 3 and 4). Thus we see Lyell destroying the connotation of his own name, and advising his friends to apply it to an entirely different geological period.

The next important step taken in discussing the later English Tertiaries was that consequent upon the labours of Searles Wood and Harmer, combined with those of S. P. Woodward. This led to another extraordinary somersault in nomenclature. Lyell himself and Sowerby had, after an examination of the earlier crag shells, decided that 26 per cent. of them only were recent. Now it was found that even in the oldest and earliest of the English crags the proportion of living exceeded that of extinct forms. Therefore, according to the absurd criterion of Deshayes and Lyell, which had been responsible for the various chameleon changes I have described, the older crag beds were again transferred from the Miocene horizon to the Pliocene, and made to represent the older Pliocene. This change was dependent on an examination of the Testacea.

Meanwhile Owen and Lankester pronounced the Mammalian remains in the Suffolk or Red Crag to be very distinctly Miocene in character, adding another element of confusion to the already interminable tangle.

Lyell, who in his original scheme had treated his Newer Pliocene as non-existent in England, having thus appropriated his older Pliocene to the Coralline and Red Crags, now proceeded to evict the Norwich Crag and associated beds, including in his scheme the Forest Bed from the Older Pliocene, and transferred it to the Newer Pliocene. To the same series he also transferred the whole of the Glacial beds, and reserved the term pleistocene apparently entirely for the beds containing palaeolithic implements, and those in which the Mammoth and its companions were found associated with human remains, which beds he supposed lay over the so-called drift, a view against which some of us have protested for years, not a word being said or suggested about

any marine equivalents of these beds. He further reserved the term Recent for everything above the so-called palaeolithic beds (*Antiquity of Man*, p. 260).

This is not enough. When we turn to a still later authority, Mr Horace B. Woodward, we find him following the lead of Mr Searles Wood, junior, and transferring the whole of the so-called Glacial series to the Pleistocene horizon, and retaining the Forest Bed and the Norwich Crag in the Newer Pliocene. With these two he also placed the Suffolk Crag, which for the first time was divorced from the Coralline Crag, and placed in another main division of the Tertiaries, the latter being left alone in the Older Pliocene. In the same work we are told very distinctly, and I think rather rashly, that Miocene beds do not occur in England and Wales at all. I am weary and tired of my imperfect analysis of this most extraordinary story. Nothing surely in all the annals of Science, nothing in the prodigious memory of my good kind friend Sherborn (may the sun long shine brightly on so industrious a worker) can equal this record, for it is not a question of naming a species, but of arranging and naming a great series of geological horizons. The chameleon has adopted every possible tint. The names I have quoted have done service in every possible variety of way, and have had as many meanings as there are whims in women. It is simply astounding that such a mass of absurdity as is involved in the story I have told should have been tolerated. It has only been so tolerated, I take it, because men have been afraid to say that Lyell, great genius as he was, has in this matter, as in many others, led the geological rabble into the wilderness; has led not merely the geological lambs, but the old horny rams and hornless ewes as well. Anyone who has favoured me by reading what I have here written is at liberty, if he can, to find some rational clue to the whole mess and maze, and thus to justify it. To me the names I have quoted have been used in so many different senses, apparently at the whim of fancy of every new writer, and have such an absolutely confused connotation that they are completely worthless for all serious purposes; and much as I dislike changes of nomenclature, I see no possible hope save to sweep them away into the limbo of oblivion.

But the names are a small matter. I protest also, as a heretic should, against the whole scheme and method of Lyell's systematic arrangement of the Tertiary and Recent beds as utterly irrational, and based upon entirely misleading and mischievous criteria, namely, the proportion of recent to fossil forms in any bed, and I will quote, in again doing so, a former president of the Geological Society, a big man with a very wide grasp of logic and knowledge. *Apropos* of this very scheme, Whewell, in his address to the

Geological Society, said: "I conceive that in a matter of arrangement any arbitrary numerical character must lead to violations of nature's classification, and can only be considered as an artificial method to be used provisionally till some more genuine principle of order is discovered" (*Proc. Geol. Soc.*, ii. 637).

Thirdly, I protest again against attempting to force false analogies and inferences into our science by giving a common name to beds so far separated as are the Tertiary beds of the Mediterranean, those of Touraine, and those of the borders of the North Sea. The equivalents of our crag are not to be sought in Sicily, or in Piedmont, or on the Loire, but in Belgium and other places on the North Sea, where common features will justify a common nomenclature. The different geological basins or provinces here named should be studied separately, and their beds should have a separate nomenclature; so should the subaerial and submarine beds in each. My present paper is critical. My next one, if you will have it, shall attempt something constructive.

HENRY H. HOWORTH.

II

The Broadening of Atoll-islets

A POETIC simile of Dana's compared an atoll to a garland thrown upon the waters. But few are the atolls that answer to this ideal; in most the wreath encircling the lagoon is twisted and torn to fragments, forming a more or less complete chain of atoll-islets.

That the shore line of these islets is not stationary has long been recognised. By an extension of their length they may unite into a complete ring, and further growth upon the inner sides may result in the obliteration of the lagoon and the conversion of the atoll into an island.

Murray[1] allows that in small atolls the islets may thus by ingrowth fill up the lagoon, yet he considers that in atolls over two miles in diameter the lagoon would enlarge by solution, which implies that the lagoon shores of the islets would be eaten back.

This idea has not received support from those who have studied coral reefs on the spot in either the Indian, Atlantic, or Pacific Oceans. Guppy[2] has described in admirable detail the way in which the large lagoon of Keeling Island is gradually disappearing, Heilprin[3] has pertinently remarked of the Bermudas that whereas we should expect an exposure of bare rock to occur in a basin of solution, his dredgings invariably proved the entire floor to be covered with a thick deposit of ooze. To me[4] the wide lagoon of Funafuti seemed on every side to be encroached upon by the land. On the other hand, Bourne observed in Diego Garcia that the lagoon made inroads on the land.

While attention has thus been paid to the increase or decrease of the land upon the lagoon shore of an atoll, less notice seems to have been bestowed on the possibility of fluctuations on the outer or peripheral coast. Some features of the Funafuti beach suggested to me a seaward growth.

Crossing the largest or windward islet from east to west, that is from the ocean to the lagoon, there appears first a steep and

[1] *Nature*, vol. xlviii., 1893, p. 576.
[2] Guppy, *Scottish Geogr. Mag.*, vol. v., 1889, p. 573.
[3] Heilprin, "The Bermudas Islands," 1889, p. 44.
[4] Hedley, "The Atoll of Funafuti," 1896, p. 17.

high bank of coral shingle facing the waves, then a gentler, longer slope inland, after this another shingle bank, less steep and less high, followed by a similar slope, then another and perhaps still another bank, each lower than its predecessor, the last scarcely perceptible. The coral blocks of which these banks are built vary according to their situation; on the seaward side they are pebbles and boulders, mostly so ground and worn as to be scarcely recognisable as corals, but on the banks further inland they are rough and much etched by the weather, there they are also brittle from decay.

The history of these successive banks of coral debris seems obviously to be, that each was piled up by the ocean waves and was afterwards shut in by its eastern neighbour, built up to windward at a later date. The lower present level of the inner banks must be the result of collapse of its material, comparable to the collapse that occurs in an unused heap of road metal, while the more advanced state of decay of that material, previously noted, likewise points to its superior antiquity. While the upper parts of the hurricane beaches thus change, their lowest stratum may by solution and deposition be welded into breccia, as a heap of hail may be turned by slight melting and freezing into a solid mass.

These wave-like rows of shingle appear to be an ordinary phenomenon of atoll structure and have been noted by many observers. Guppy[1] saw on the windward angle of Keeling Island a massive looking slope of large blocks of coral forming a kind of glacis. On North Keeling Island he was informed that the shore on the landing place on the west coast had advanced some fifteen or twenty paces during the previous ten or fifteen years, the old line being indicated by the overhanging cocoanut palms removed that distance from the sea; it was largely the work of a single night, a huge pile of coral blocks being piled on the beach during a cyclone.

Speaking of the atoll of Peru, one of the Gilbert or Kingsmill Archipelago, Whitmee[2] says, "The Island itself is formed of successive ridges of sand, broken coral, and shells. These ridges are most of them from thirty to fifty feet across, and the hollows formed between them are generally from four to six feet in depth. For some distance, at that end of the island which I examined, they run across, and in the middle they run parallel with the sides of the island. The whole extent examined presented the same appearance, and the ridges were so regular that they gave one the idea of being artificially formed. The waves must exert a mighty force during heavy weather to form these extensive ridges.

[1] Guppy, *Scot. Geogr. Mag.*, vol. v., 1889, pp. 296 and 468.

[2] Whitmee, "A Missionary Cruise in the South Pacific," 1871, p. 35.

There is little doubt but each ridge is the result of a single storm. I have already referred in the notice of Atafu in the Tokelau group to a similar ridge of smaller dimensions which was thrown up during the present year, and I have seen several small islands of broken coral and shells which were formed on the reefs in Samoa during the hurricane of a few hours' duration."

The most remarkable passage which I have read in the literature of these successive hurricane beaches I have spaced in the following quotation. Describing Maldon Island, an isolated atoll in the Central Pacific, Dixon[1] relates that at some unknown epoch the atoll was inhabited by a colony of Polynesians and was afterwards deserted: he continues: "Opposite all the kitchen heaps along the north and south banks and at some intermediate places, the ridges had been levelled to form pathways to the reef, and flat slabs had been laid down, forming a line of stepping stones. The cuttings and stepping stones extended over the six inner ridges, whilst the three outer ones were invariably as formed by the waves, forming a record at present unreadable of the desertion of the island."

A geologist will at once seize on the fact that here is evidence that the islet has increased by one-third of its breadth since the recent date of the departure of the Polynesians. Further reflection will suggest that a continuation of the process of building line after line of hurricane beaches would encroach upon the reef flat until these beaches reached its seaward edge. Yet the reef flat, by the unanimous testimony of observers, always preserves a given width according to the longitude, or rather, as Dana hints, perhaps according to the tide of the locality.

If the facts above adduced be accepted as showing that the hurricane beaches tend to steadily and rapidly broaden the atoll islets to windward, then there is no escape from the deduction that to preserve the normal width of the reef-flat, that too must be steadily growing seaward at the same rate. Presumably the reef flat advances on a talus of its own debris. Some intrinsic evidence in support of the advance is, I think, furnished by the reef flat itself.

Dana was the first to point out that in the west central Pacific a characteristic feature of the reef flat is the presence of numerous fissures reaching almost to the beach.[2] At Nui, Whitmee observed that "the seawater gains access to the central lagoon through the reef underneath the islands. In some it bubbles up in the midst of the lagoons, forming immense natural fountains."[3] Gill, in describ-

[1] W. A. Dixon, *Trans. Roy. Soc., N.S.W.*, 1877, p. 175.
[2] Dana, "Corals and Coral Islands," 1872, p. 186.
[3] In article "Polynesia," *Encycl. Brit.* (9) xix., 1885, p. 420.

ing his visit to the atoll of Mitiaro, Hervey Group, says, "Towards midnight we were alarmed by a strange rushing of waters underneath. Next day we were told that it was the strong influx of the sea by a subterranean passage into the lagoon."[1] The same writer has mentioned great submarine caverns in the reef, the haunts of large sharks, which are pursued even to these recesses by the daring Polynesian fishermen.[2] Such caverns form everywhere a conspicuous feature in upheaved coral reefs.

On Funafuti the coral on the margin of the reef flat appeared to me to grow seawards in piers; as these broaden their interstices first form wide trenches, then narrow crevasses that may be stepped across, which clefts tend to be roofed in by growth of Nullipores and are narrowest at the surface, ultimately, proceeding inshore, they become mere fissures and then disappear. This disappearance only refers to the surface, for they probably form tunnels far into the centre of the islet, as shown by the openings through which the sea floods the mangrove swamp.

Either these caverns and tunnels, shown by the foregoing quotations to be a usual feature of coral reef growth, were excavated out of the solid rock by the sea in some unknown way, which seems an improbable hypothesis, or the caverns represent spaces enclosed by growth of coral. On the latter supposition they must have been so enclosed when the margin of the reef flat lay further and further inland. Consequently they are being still enclosed as that margin grows further and further seaward, and the shelf of reef flat, as was sought to be proved, thus widens for the reception of successive hurricane beaches.

On the leeward side of an atoll the shingle beaches are absent, their place being occupied by blown sand. The arrangement of this material does not afford data of age and growth like that presented by the shingle. Analogy will, however, suggest that the leeward margin keeps pace with the peripheral extension of the atoll to windward, the spasmodic increase of the hurricane beaches being probably represented by a gradual aggregation of blown sand.

Summary.—The islets of an atoll have been shown by modern investigators, especially by Guppy, to be increasing laterally, and usually at a stationary level. These observations chiefly applied to the inner or lagoon beaches. At the windward corner of an atoll concretionary lines of shingle beach are frequently, perhaps usually, arranged. Because each is the result of a cyclone, these are appropriately termed hurricane beaches. Like the rings shown in a transverse section of a tree, these hurricane beaches may supply chronological data. A continuation of beach building would bury

[1] Gill, "Life in the Southern Isles," 1876, p. 173.
[2] Gill, "Jottings from the Pacific," 1885, p. 149.

the reef flat; that it is not so buried suggests that the reef flat as well as the dry land advances seawards. The reef flat is typically seamed with channels; if these are not excavated by the sea (which is incredible) then they must be enclosed by coral growth. That they are so enclosed is offered as proof that the reef flat itself grows seawards, built out probably on a foundation of its own waste.

CHARLES HEDLEY.

AUSTRALIAN MUSEUM, SYDNEY.

III

Second Contribution on Numerical Variation of parts in Ranunculus repens (L.)

THE present investigation was undertaken in continuation of a paper which appeared in *Natural Science* for May 1897, p. 323, as it was thought that the conclusions drawn and results obtained from a series of 500 specimens, might be influenced by the small number examined. Accordingly (as noted in *Natural Science*, June 1897, p. 429), another series of 500 was examined, and in the following paper the results of this second series, separately, and combined with the first, are discussed. It will, I think, be admitted that conclusions based on observations of 1000 specimens are fairly well founded. The specimens were again most kindly supplied by Miss K. M. Hall, Curator of the Whitechapel Museum, from practically the same locality as last year, my occupation preventing me from collecting them myself.

The number of specimens examined was again 500. The method employed in recording the results obtained was slightly different from that used last year. Sheets of paper ruled in five columns were taken, and the columns were marked successively—first column, consecutive numbers from 1 to 500; second column, calyx; third, fourth, and fifth, corolla, stamens, and carpels respectively, each specimen having one line to itself on one sheet.

Example—

1896.

Number.	Calyx.	Corolla.	Stamens.	Carpels.
1	5	5	68	46
2	5	9	55	43

With this method the results are more accessible than with the former plan, which, however, possessed certain advantages.

The total number of parts in the 1000 specimens examined was—

	1896.	1897.	Total.
Calyx . . .	2,502	2,496	4,998
Corolla . . .	2,836	2,717	5,553
Stamens . . .	27,267	26,808	54,075
Carpels . . .	18,037	17,354	35,391
Total .	50,642	49,375	100,017

It will be seen that these figures show an average of rather more than one hundred parts (organs) to each specimen.

The time occupied in the actual dissection of the specimens was in the aggregate nearly 56 hours, this giving each specimen, on an average, about $3\frac{1}{3}$ minutes.

The floral formula obtained from last year's work was—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 36.

This year's results are—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 35 :

and the mean of the two—that is to say, the floral formula obtained by calculating the mean of 1000 specimens, is—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 35.

The results are given exactly, in tabulated form, below.

	Mean of Calyx.	Mean of Corolla.	Mean of Stamens.	Mean of Carpels.
1896 . .	5·004	5·672	54·534	36·074
1897 . .	4·992	5·434	53·616	34·708
Mean . .	4·998	5·553	54·075	35·391

It will be seen that the corolla shows a marked tendency to increase the number of its members, more so in the first series than in the second. In fact, in view of the large fraction, the corolla might almost be given as 6. Four specimens in the 1000 conform to the formula given—two in the first series (1896) and two in the second series (1897).

Calyx.

	1896.	1897.	Total.
Number of specimens with 3 sepals	—	1	1
" " 4 "	9	11	20
" " 5 "	481	478	959
" " 6 "	9	9	18
" " 7 "	1	1	2

In the one specimen having but three members to the calyx the sepals were equidistant from each other.

In all 20 cases of 4 sepals there was a re-adjustment of

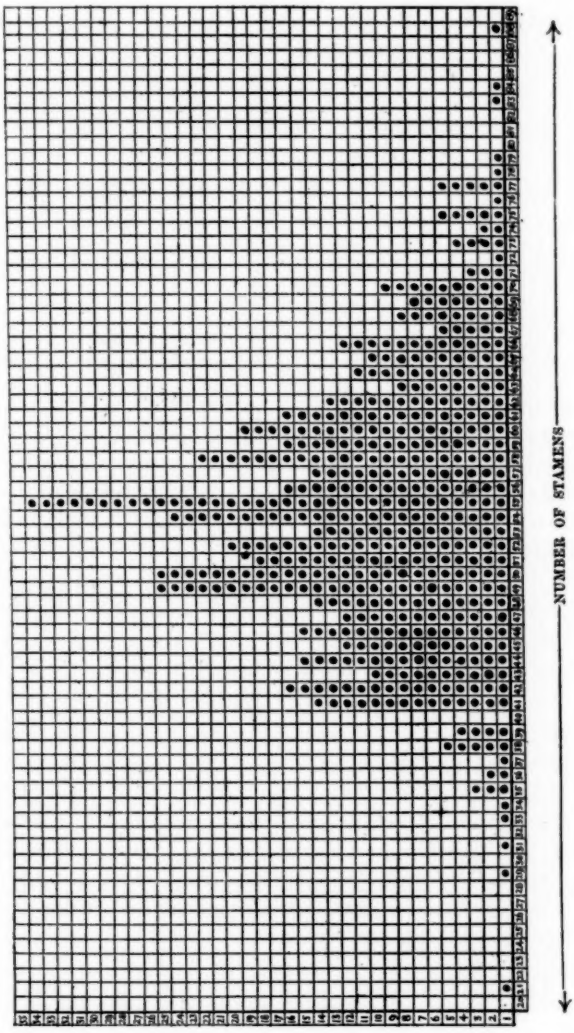


FIG. 1.—VARIATION IN STAMENS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.)

symmetry, and in 16 of the 18 cases of 6 sepals there was also a re-adjustment of symmetry.

It will be observed that the results of the second 500 show, as in the first series, that very little variation occurs

in this (calyx) whorl, the two series agreeing most remarkably.

The total amount of variation from the mean being so small (4·1 per cent.), it may be stated that there is practically no varying in the number of parts.

Corolla.

	1896.	1897.	Total.
Number of specimens with 4 petals	7	1	8
„ „ 5 „	345	361	706
„ „ 6 „	60	85	145
„ „ 7 „	36	36	72
„ „ 8 „	24	14	38
„ „ 9 „	13	2	15
„ „ 10 „	6	1	7
„ „ 11 „	7	—	7
„ „ 12 „	1	—	1
„ „ 13 „	1	—	1

In the second series there is, on the whole, rather less tendency in the petal whorl to vary from the mean than was shown in the first series—72·2 per cent. of the second 500 having the average number of petals, against 69 per cent. last year. The number of specimens having 6 petals is, however, considerably greater than in the first series. By a curious coincidence the number with 7 petals is the same in both series (36). The greatest difference between the two, lies in the specimens having more than 7 petals, the first series having 52, the second only 17.

Stamens.

On reference to fig. 1, which represents the variation in the first series of 500 specimens (those examined in 1896), it will be seen that the curve is fairly regular, there being, however, a decided hump at 49-50. Fig. 2 gives the corresponding curve for the second 500 (1897). This is very much less satisfactory, there being no decided maximum, as in fig. 1, and, also, the recorded maximum 47 does not agree with the calculated mean 54.

The range of variation is—

Series I.—Maximum 88, minimum 21.
Series II.—Maximum 84, minimum 23.

Fig. 3 gives the curve obtained by combining the two series. It must, I think, on the whole, be considered very satisfactory. The recorded maximum is 55, the calculated mean 54. The hump at 49, remarked in fig. 1, is still noticeable, although not traceable in fig. 2.

Carpels.

The extreme irregularity of this curve given by the first series (fig. 4) was noticed in my first paper, the recorded maximum being at 25, while the calculated mean (for that series) is 36,

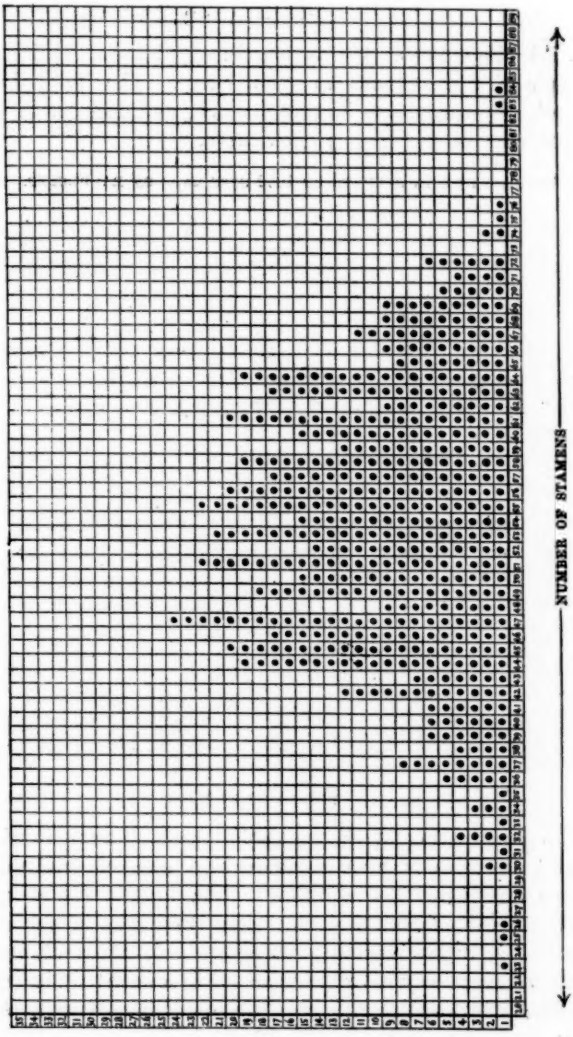


FIG. 2.—VARIATION IN STAMENS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *Second Series.*

a marked secondary and also a tertiary maximum being noticeable.

The curve of the second series of carpels (fig. 5) shows a

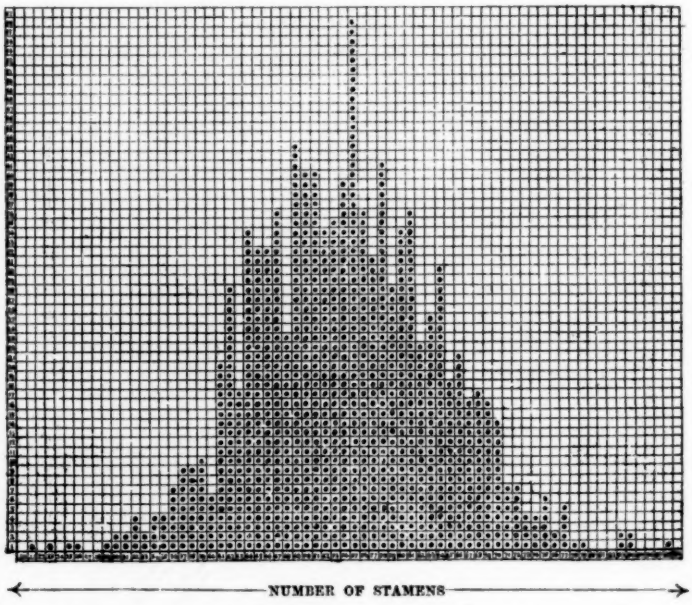
regularity which, with 500 specimens, I think would be hard to surpass. The recorded maxima are 34 and 36—the calculated mean, 35.

The range of variation is very much less in the second series than in the first.

Series I.—Maximum 65, minimum 15.
Series II.—Maximum 52, minimum 17.

Fig. 6 is the curve of the two series combined. It is a very fair example of a continuous variation curve. The hump in fig. 4 at 25 still, however, makes its presence felt, and destroys the symmetry of otherwise an exceedingly good curve.

FIG. 3.—VARIATION IN STAMENS OF 1000 SPECIMENS OF RANUNCULUS REPENS (L.)



On examination of the appended charts I think it will be conceded that there is here an undoubted case of undisturbed continuous variation. The addition to the first, of the second series, simply rounds off and does not alter the curve in general. Any irregularities in the resultant curves are only apparent, and are due to the comparatively small number of specimens under notice. I consider, from these results, that the concluding remark of my first paper must be answered in the affirmative.[1]

[1] I suggested (*Natural Science*, May 1897, p. 328) that the recorded irregularities in the stamen and carpel curves were only due to the small number of specimens (500) then under discussion, and that they would disappear if a larger number were examined.

Correlation of Variation

Calyx

In every case of calyx variation, whether above or below the mean, all the other whorls are above the average. As there are, however, only 41 specimens out of the 1000 to prove this, I do not think that much importance should be attached to the results.

Formulae obtained from specimens having variation in calyx—

Minus variation

	Petals.	Stamens.	Carpels.
Specimens with 3 sepals . .	5·0	59·0	41·0
" " 4 " . .	6·0	50·5	34·5
Mean . .	5·5	54·7	37·7

Plus variation

	Petals.	Stamens.	Carpels.
Specimens with 6 sepals . .	6·3	52·3	36·0
" " 7 " . .	6·0	62·5	40·5
Mean . .	6·1	57·4	38·2

Collective variation

	Petals.	Stamens.	Carpels.
Mean of specimens with 3 sepals .	5·0	59·0	41·0
" " 4 " .	6·0	50·5	34·5
" " 6 " .	6·3	52·3	36·0
" " 7 " .	6·0	62·5	40·5
Mean .	5·8	56·0	38·0

Calyx and Corolla

Analysis of variation in calyx and corolla—

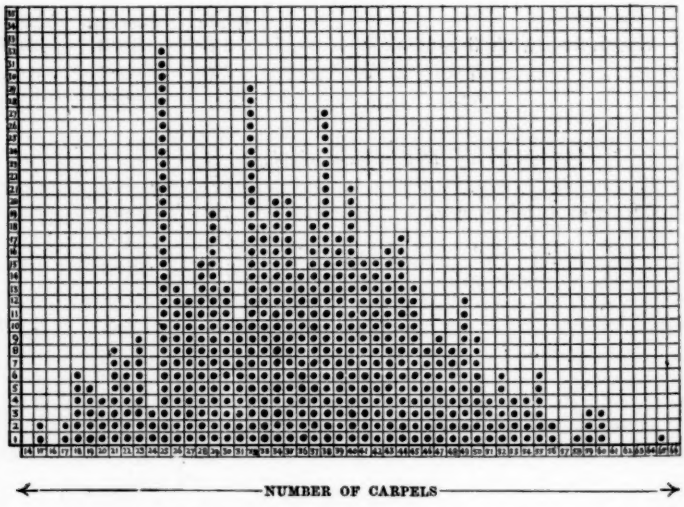
Number of specimens having in			1896.	1897.	Total.	P. cent.
calyx	5 sepals, and in corolla	5 petals	338	351	689	68·9
5	" "	less than 5 "	7	1	8	·8
5	" "	more than 5 "	136	126	262	26·2
Less than 5	" "	5 "	4	4	8	·8
" 5	" "	less than 5 "	...	...	...	...
" 5	" "	more than 5 "	5	8	13	1·3
More than 5	" "	5 "	3	6	9	·9
" 5	" "	less than 5 "	...	...	...	...
" 5	" "	more than 5 "	7	4	11	1·1

The calyx having practically no variation, it appears from the above that if there is any superabundant calyx- or corolla-material in the embryonic flower, rather than increase the two whorls alike, the sepals are left unaffected, and the superabundant matter goes to form more petals—third line of above table. Actually, the calyx and corolla whorls of the specimens represented in that line have a

combined average of 12 parts, but instead of each having 6, as we should assume from the formula being normally 5 and 5, the calyx remains the same, 5, and the corolla rises to 7. In further analysis of the last statement, the mean of the 262 specimens in which the calyx had 5 sepals and the corolla more than 5 petals, is—petals, 6·9; stamens, 52·4; carpels, 37·4. The extra material available has increased the corolla by 1·9 members (or more strictly by 1·4), and has also added to the carpels; but in these specimens the stamens are decidedly below the general mean.

We may assume therefore, from the foregoing, that if there is any plus variation in any specimen, the chances are that the calyx will remain normal, the corolla and carpels be increased, while the stamens will be below the mean.

FIG. 4.—VARIATION IN CARPELS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *First Series.*



(The formula given by the 8 specimens with corolla below the mean is—calyx, 5; corolla, 4; stamens, 55·3; carpels, 40·5.)

CORRELATION OF VARIATION IN STAMENS AND CARPELS

Number of specimens having				1896.	1897.	Total.	P. cent.
stamens	54	and carpels	35 .	2	2	4	·4
	54	„	less than 35 .	9	5	14	1·4
	54	„	more than 35 .	13	8	21	2·1
Less than	54	„	35 .	10	15	25	2·5
„	54	„	less than 35 .	136	183	319	31·9
„	54	„	more than 35 .	84	50	134	13·4
More than	54	„	35 .	8	12	20	2·0
„	54	„	less than 35 .	82	59	141	14·1
„	54	„	more than 35 .	156	166	322	32·2

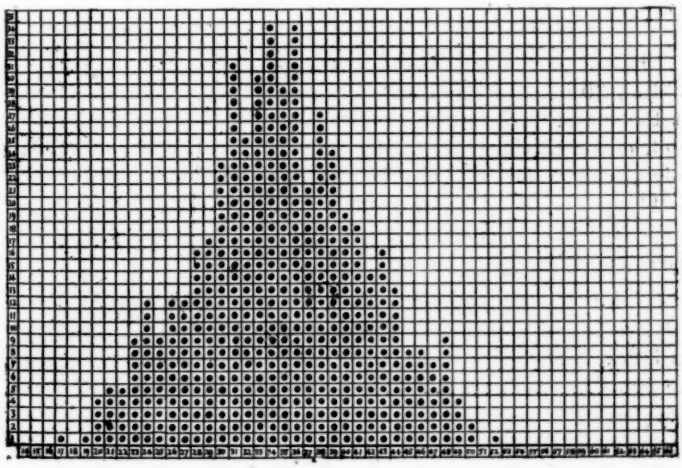
From these figures I assume that there is only a certain amount of sexual material to begin with, and if this is insufficient to form the average number of stamens, the number of carpels will also be below the mean. The reverse also holds good—that is, if there is a superabundance of sexual matter, so that the number of stamens is above the mean, the number of carpels will also be above the mean. We do not find, at least not to any extent (1·4 per cent.), that the number of stamens comes up to the mean at the expense of the carpel whorl, or that the carpels increase at the expense of the stamens (2·5 per cent.).

Correlation of Variation in all four Whorls collectively

There are 81 possibilities of such variation (3^4)—'the mean,' 'below the mean,' 'above the mean'—in each of four whorls.

In the combined series (1000 specimens) of these 81 possibilities, only 35 (43 per cent.) occur.

FIG. 5.—VARIATION IN CARPELS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *Second Series.*



NUMBER OF CARPELS

It may, therefore, be definitely stated that the whorls do not vary independently. Particulars of the 35 are here given.

(1. signifies the mean, 2. = below the mean, 3. = above the mean, so that 1.1.1.1 means that the specimen has in the calyx, corolla, and stamen and carpel whorls, each respectively, the mean number of members. Similarly 1.1.1.2, for instance, signifies that calyx, corolla, and stamens are at the mean, but the number of carpels is below the mean, and so on.)

1.1.1.1.	2.1.2.2.	3.1.2.2.
1.1.1.2.	2.1.2.3.	3.1.2.3.
1.1.1.3.	2.1.3.2.	3.1.3.3.
1.1.2.1.	2.1.3.3.	3.3.2.2.
1.1.2.2.	2.3.2.2.	3.3.2.3.
1.1.2.3.	2.3.2.3.	3.3.3.1.
1.1.3.1.	2.3.3.3.	3.3.3.2.
1.1.3.2.		3.3.3.3.
1.1.3.3.		
1.2.2.2.		
1.2.2.3.		
1.2.3.2.		
1.2.3.3.		
1.3.1.3.		
1.3.2.1.		
1.3.2.2.		
1.3.2.3.		
1.3.3.1.		
1.3.3.2.		
1.3.3.3.		

It will be noted that the greater number of the observed possibilities, 20 out of 35 (57 per cent.), occur when the calyx whorl is at the mean. Of course, recollecting that in (practically) 96 per cent. of the specimens there was no variation from the mean in the calyx, such would naturally follow. Of the remaining 4 per cent. —41 specimens—15 have purely chance variations, a rather less proportion than that of the whole series—36 per cent. of 41 specimens against 43 per cent. of 1000.

I have now shortly discussed correlation of variation in calyx,—calyx and corolla,—stamens and carpels,—and lastly,—correlation of variation in all four whorls collectively. To summarise the results, we found in the analysis of the *calyx* that increase in corolla was in every case accompanied by increase in the stamens and carpels independently of the calyx. Therefore, as regards the calyx on the one hand, and corolla, stamens, and carpels collectively on the other, the data obtained seem to show that the number of members of the calyx has no effect whatever on the other three whorls. In this instance at least, any variation is pure chance. It may be, however, that this conclusion should be modified on account of the small number of specimens affecting it.

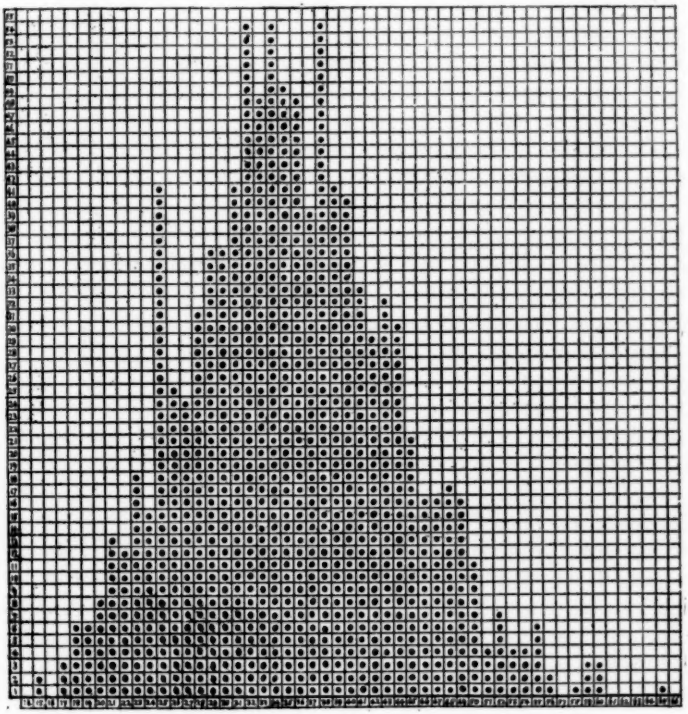
Calyx and Corolla

The results obtained here are rather perplexing, but in view of the number of specimens discussed (262), I presume, must be accepted.

Stamens and Carpels

No further remarks are needed, as the figures given are sufficiently convincing. The data given of the *collective correlation* are equally, or perhaps more, conclusive, and do not require further discussion here.

FIG. 6.—VARIATION IN CARPELS OF 1000 SPECIMENS OF RANUNCULUS REPENS (L.)



In concluding this paper I must express my thanks to Miss K. M. Hall for supplying the specimens, and to Mr P. Chalmers Mitchell for making various suggestions.

JOHN H. PLEDGE.

115 RICHMOND ROAD, LONDON, N.E.

IV

Egyptian Fisheries

IN Egypt as in other Mohamedan countries fish forms an important part of the food of the people. Fish diet is not enjoined upon the followers of Islam by their religion, but, from firmly established ideas as to its virtues, its use is universal amongst the rich and poor of the country, and it follows that the fishing industry is one of considerable importance.

The Nile itself and the network of irrigation canals throughout the cultivable parts of the Delta furnishes the adjacent population with a supply of food. In the case of the larger towns and more populous districts this supply is augmented by fish from Birket El-Keroun, the supposed residue of the ancient Lake Moeris, from the brackish lakes on the northern coast line and from the Mediterranean and the Red Sea. The local supply from the Nile and its canals, in fact all fresh-water fish, is as a rule eaten fresh, and the same may be said for the marine supply coming from Suez and Alexandria; that from the brackish lakes consists mainly of salted fish, a very small proportion of the lake fish being sold for fresh consumption in the immediate neighbourhood. The reason for this is due partly to the difficulty of transport, there being no quick service of trains nor means for keeping fish fresh over a long railway journey, but the chief reason is the strong demand there is for a form of salt fish called *fessikh*, made from grey mullet, the predominating fish of these localities. Of the Nile species all are eaten fresh with the exception of *Tetrodon fahaka* and *Malapterurus*, the poorer natives being the chief consumers. The flesh of all is more or less muddy tasted or insipid. Serranidae and Carangidae furnish the bulk of the edible species from the Red Sea, and all the larger sized forms of the littoral fish fauna are to be found in markets at Alexandria and Port Said. The fresh water fishermen are, all of them, natives, who form no distinctive class of the population. The sea-fisheries are pursued chiefly by Neapolitan fishermen, who make Alexandria, Port Said and Suez their headquarters. Native fishermen are to be found at these places, but they seldom venture far from the shore. At Lake Menzaleh, the largest and most important of the brackish lakes, the fishing population is quite distinct from the *fellaheen* or agricultural labourers. They live on two islands in the lake, and with few excep-

tions they marry among themselves and they hold native outsiders in great contempt. Their physique is fine and their cast of countenance is distinctly Caucasian, indeed, some authorities have it that they are descendants of the Hyksos or shepherd kings of Ancient Egypt.

The brackish water fisheries, as being most characteristic of the country and being those with which I have been most intimately associated, I shall describe at some detail. The brackish lakes, beginning at the extreme eastern boundary of Egypt and going westwards are Lakes Birdaweel and Umm Farag between El Arich and Port Said, Lake Menzaleh between Port Said and Damietta, lakes Bouroullos and Edkou are between Damietta and Alexandria, and Lake Mareotis behind and to the west of the latter place. The two first mentioned receive no fresh water feeders, and on this account their sea entrances, on which they depend for their existence, often become silted up and require to be kept open by the tenant of the fishings. The fauna is entirely marine, so they can scarcely be placed under the present category. On the other hand, Lake Mareotis, although it receives fresh water from numerous canals or drains, does not communicate directly with the sea, the pumping apparatus required to keep the level of its waters low preventing the entrance of marine forms. The three other lakes each possesses a sea entrance, and is connected with the Nile system through receiving the drainage water and flood discharge of several irrigation canals. Lake Edkou was, till it was closed last year, fished in a manner similar to the *valli* of the Italian Adriatic. The sea entrances of Lakes Bouroullos and Menzaleh remain unobstructedly open and are centres of much activity when the spawning instincts of the lake fish cause them to move seaward. Taking Lake Menzaleh as the type, it is shallow, its depth on an average being about 140 c.m., but in the neighbourhood of the sea entrance, there are several converging channels of a depth varying from 3 to 6 metres. A current flows constantly through the entrance with a direction and strength governed by the state of the tide, the direction of the wind, and the inflow of fresh water from the canals.

The fish fauna consists of a mixture of fresh-water and marine forms, of which the latter predominate, and their relative distribution is regulated by the density of the water, which varies in the several parts of the lake at different seasons. With the exception of the Mormyridae, all the families of Nile fish are represented and two species at least breed in the lake. These are *Chromis niloticus* and *C. menzalensis*, and while the former spawns in the neighbourhood of the fresh-water inlets, the latter breeds in water as salt as, if not salter than, the sea itself. These two forms and the *Silurid Clarias*, if we exclude *Anguilla*, are the constant members of the fresh-

water species, the occurrence of others being more or less accidental. At the period of high Nile the lake receives numerous additions to the numbers of its fresh-water species. Of the marine fauna, the Mugils are the most abundant. Five species are found in the lake, and two of these—viz., *M. cephalus* and *M. capito*—are of the highest economic importance. They are taken at all seasons of the year, though at certain periods more abundantly than in others, due either to their local occurrence during the spawning season or to certain conditions of the water. *M. capito* is more generally diffused throughout the lake than the larger species, and it shows a greater preference for fresh water than the latter, which in the adult stage is always found near the deeper and salter localities. Neither of them attain the maximum recorded size of their species. *M. capito* is rarely found to exceed 40 c.m. in length, and 58 c.m. is about the maximum for *cephalus*. This would appear partly to be the result of overfishing, but observations show that in the case of *cephalus* few spent individuals return to the lake after spawning is over, and that nearly all the fish of the spawning shoals which leave the lake are virgin fish. Older fish are to be found in the Suez Canal, which has yielded individuals of over 84 c.m. The spawning season of *M. cephalus* lasts from May till the beginning of November, a period which seems to be interrupted for from six to eight weeks in the months of August and September when only a few scattered spawners leave the lake for the sea. *M. capito* spawns from the first week of November till about the end of December. They leave the lake in shoals, which return empty after one day or two, then after an interval, during which their reproductive organs fill again, they migrate seawards a second time. At least two crops of ova are ripened in a spawning season.

As before mentioned, the grey mullets are used for the manufacture of fessikh, and they form on an average nine-tenths of the total catch landed at the markets.

Next in importance come *Morone labrax* and *M. orientalis*. They spawn from about the end of December till March, the season of the smaller fish lasting the longer of the two. *M. labrax* is in little evidence and *orientalis* not at all during the months of summer, but from October till the end of the spawning season they are caught in large numbers.

Sciena aquila and *Umbrina cirrhosa* are found in the lake throughout the year. They would appear to spawn sometime in May or June, but no ripe specimens have been collected.

Chrysophyrs aurata, which is found in the salter parts of the lake, leaves to spawn during the months of November, December and January.

The annual spawning migration of the common eel takes place

in December, January and February, when the moon is in her last quarter. They leave after sunset or in the late afternoon should the sky be overcast.

Other forms occur less commonly as *Serranus cabrilla*, *Hemiramphi*, and some Carangidae, while among the more accidental may be mentioned *Balistes*, which has been taken in good condition ten miles from the sea entrance.

Fishing is usually prosecuted from boats which are manned, on an average, by a crew of four men not including boys. They fish either singly or in companies of as many as fifteen boats. Nets and hooks, baited and naked, are used. Of nets, the mullet seine is that in most general use. It is carried round a shoal of fish, which has been driven towards it by men who support the upper edge of the net about three feet above the surface of the water by means of poles to prevent the escape of the fish. In the Suez Canal this system cannot be employed on account of the depth of the water, but a trammel net supported horizontally on reeds is attached to the outer edge of the seine and receives the fish as they jump the corked line. Fixed trammels, trammel seines, and drag nets are also used, while the circular casting net is a great favourite with the fishermen, who are very adept in its use.

Much damage has been done to the fisheries by denying to them the protection of properly constituted bye-laws. Till two years ago no attempt was made to regulate the capture of fish, and even now the size of mesh used is that which suits the convenience of the fishermen. Lately it has been forbidden to fish on grounds which were ascertained to be the nurseries of young fish; formerly forty or more boats' crews would combine to sweep the fish, chiefly young grey mullet of an average size of 17 c.m., from the lagoons on the margin of the lake which fish of that size frequent. As much as fifteen tons of these fish have been taken in a single haul. A close time of eight days has been enforced during the last two spawning seasons of *M. cephalus* for the protection of this fish, and the indications of success following the institution of this and other protectionary measures promise, under proper control, a great future for the industries in question.

J. C. MITCHELL.

V

The Red and Blue Colouring Matters of Flowers

IT is rather difficult to refer to an exact date the sober beginnings of our present knowledge regarding the chromogens or the visible colouring matters of flowers and leaves. Schubler and Decandolle endeavoured to prove the existence of two essentially different series of flower colours, viz., the *xanthic* producing the yellow tints with their transitions into red, and the *cyanic* producing the blue tints with their modifications, and that the colours of both series are formed from chlorophyll, the xanthic by oxidation and the cyanic by deoxidation. This view may, however, be considered as the result of mere conjectural surmise rather than that of a definite chemical investigation. The researches of Filhol, Cloez, and Fremy served to elicit the distinction between the blue and red pigments which are soluble in water, and many of the yellow which are of a resinous nature and dissolve only in alcohol and ether. The blue pigment (cyanin or anthocyan) with acids makes red flowers, alkalies turn it green ; and hence it was concluded that the blue colours of flowers are not produced from the red by the action of alkalies. They found that blue, violet, red, brown, and orange flowers have only one colouring matter, while yellow flowers have two, viz., xanthin insoluble in water, and xanthein soluble in water. According to Cloez and Fremy all red and rose flowers have an acid cell sap, and the colour of this sap would be due to a modification, under the influence of an acid, of a colouring matter (cyanin), which is found likewise, but in the state of greater purity, in blue flowers whose sap is neutral. Finally, it was recognised by Filhol that the flowers of poppy, *Pelargonium*, *Camellia*, and *Salvia* contain a pigment which is more stable than that of most other flowers, that they contain no resin or xanthogen, and that when treated with alkalies they assume a blue or violet colour without any green. Such were some of the earliest researches on this intricate and most interesting subject, and the fundamental error or defect which characterised them all was that the rose, blue, and violet flowers owe their tints to one and the same substance, influenced by the reactions of the vegetable juices, this substance (cyanin) itself being a blue uncrystallisable mass soluble in water and alcohol, but insoluble in ether.

This mistake is one which, in the absence of any definite knowledge anent the differences in kind and the sources of the various tannic chromogens, might have been easily made by anyone who

experimented on flower colours with alkalies and acids as his only reagents. By the employment of neutral reagents, however, such as acetates of lead and magnesium, the results are such as to afford ground for distinction between the various hues and tints. For instance, the fine green precipitate yielded by acetate of lead with the colouring matter of the rose tribe is turned red again by acetic acid, but the similar precipitate given by the blue pigment of harebell corolla is turned blue by this acid; and with acetate of magnesium and acid the effects are similar. The blue of scabious when treated with acetate of lead yields a similar green colour and precipitate, but on adding acetic acid we have a red liquid and a still green precipitate, the portion lying under the acid liquid gradually turns blue, while the portion outside the liquid, though still bathed therewith, remains green. This interesting experiment seems to clearly suggest a radical difference in the composition or constitution of the normal red and blue pigments of flowers, but likewise proves the fallacy of the early view that 'rose, blue, and violet flowers owe their tints to one and the same substance influenced by the reactions of the vegetable juices.' Similar experiments even seem to show that certain distinctively red flowers, such as camellia, owe their tint to a substance which is not the same as that of other red flowers, such as rose; but in this case there is more than a suspicion that the real and original colour of the former kind is blue and not red, and that artificial means could practically effect the change. Even acetate of lead when used alone to react on various red colouring matters of vegetable origin produces results which argue an essential diversity therein. Thus, with genuine red wine it yields a greyish-blue or greenish colour, with the colouring matter of bilberries it gives a blue precipitate, with that of mallow or elderberries a green, and with that of phytolacca berries a red-violet precipitate. V. Vogel, experimenting later, found that by using a concentrated solution of sulphate of copper the colour of new or old red wines was decolourised, although the fresh pellicle of the grape itself is coloured violet by this reagent, as is also the extract of cherry juice; while that of bilberry remained unchanged. By the action of sub-acetate of lead bilberry juice is completely decolourised, while that of cherry is not decolourised in the presence of alcohol or of red wine. The spectroscope has also been employed to distinguish between these diverse pigments, but its revelations need not be referred to here, especially as I am disposed to consider them to be not so very valuable owing to the extreme difficulty of isolating completely the different colouring matters.

A decided advance was made towards a veritable chemical comprehension of the subject when Morren declared that the red and blue

colouring matters are not formed from chlorophyll, and when Marquart observed that cyanin appeared by its characters and its little stability to approach the paracarthamin obtained by the action of sodium amalgam on quercitrin and quercetin, a discovery that immediately originated a crop of surmises, the ground whereof was, that paracarthamin, similarly as cyanin, was also turned green by weak alkalies and by acetate of lead. Thus, for example, Stein, in a short and pleasant paper to the *Jahrb. f. praktische Chemie* for 1863, distinctly avers, that in most red flowers which he examined the red colouring matter appears to be paracarthamin, and this also is the same body which is found in blue flowers; the blue pigment is none other than the saline (calcium) compound of the red matter. Nachtrag, too, in a note on the 'Natural Occurrence of Paracarthamin,' concludes, from the behaviour of the red bark of dogwood (*Cornus sanguinea*) towards alcoholic potash solution agreeing with that of the pigments of dahlia towards the same reagent, that it likewise contains paracarthamin, and he adds, "the red that can be artificially produced from plant-yellow appears, therefore, to be repeatedly present ready formed in the vegetable kingdom." A certain weight of probability was imparted to these views by the fact alluded to by Stein, viz., that quercetin had previously been shown by Rochleder to exist in the flowers of the horse-chestnut, and by the circumstance that in the flowers of *Aesculus pavia*, the origin of the red pigment from the yellow can be clearly followed, as this flower when it comes forth from the bud is yellow and only by little and little turns red. If this be really the *modus operandi* as existing in nature, it would seem to follow that the bright pigments are really reduction-products and not oxidation-products—a consequence which is not usually met with in analogous cases in other departments of biology. It is now pretty clearly ascertained that the yellow rutin or quercetin present in flowers does not, on account of the acid sap, impart a yellow colour thereto in the living condition at least, and that the tannic chromogen of the red flowers is really a development product of this or other intermediate tannoid glucoside. Moreover, it is a fact that certain tannic acids do yield on reduction with HCl and sodium-amalgam a red substance with reactions towards acetate of lead and alkalies similar to those of paracarthamin, but which nevertheless is certainly not the same as the latter body, although these same tannic acids appear to minister all the while as chromogens to the production of extremely vivid and brilliant scarlet and pure blue flower colouring matters.

The seemingly intimate relationship between paracarthamin and the red and blue pigments having thus by more explicit researches been dissolved, it became necessary for chemists to search for some other body which could possibly be isolated in sufficient purity so as

to yield the mysterious green reaction with alkalies and acetate of lead. Everybody who has commenced the study of this subject is both pleased, puzzled, and surprised, by this reaction. There is a peculiarly vivid emerald-green brilliancy about it which is almost unmatchable. I have frequently prepared paracarthamin from rutin and quercetin extracted from plants, but the reactions thereof are incomparable with those yielded by the nature-formed cyanin, whether in petal or leaf. According to Wiesner the colour of the lead salt ought to be really blue, the actual green being caused by an intermixture of tannin in the solution; but experiments with carefully purified tannin show that the colour is actually a dark bluish-green. However, to resume, it would appear that the researches of certain micro-chemists such as Nägeli, Wiesner, etc., revealed for the first time the important fact, that certain astringent matters were found in parts of plants which become red or blue, nay even the actual colouring matters seemed to spring up as it were grasped into the very substance of the tannin itself. It was natural, therefore, that Wigand should point out that the chromogen of anthocyan might be regarded as perhaps not tannin, but some modification thereof, which he denoted as cyanogen, 'the transformation depending on a process of oxidation.' This extremely valuable declaration was, however, in a most marked manner left unnoticed in later works, until Lindt detected the relationship anew, and subjoined the conjecture that the production of the red pigment might depend on phloroglucin—a decomposition product of tannin, similarly as the action of vanillin on this phenol takes place, an action which results from the combination of two molecules phenol with one molecule aldehyde with elimination of water and production of a red resinous body. Waage, however, in 1890 recognised the fact that the presence of phloroglucin in a plant was a by no means absolute requirement for the coming forth of a soluble red pigment (anthocyan), since, for instance many Chenopodiaceae are tinged strongly red, though no phloroglucin can be detected therein. It might possibly, of course, happen that on the production of anthocyan a consumption of phloroglucin took place, or perhaps it required a longer time to form in the light than the red matter which was suddenly created thereby in a colourless tissue, and hence in either case it would not be detected; but then again it may be objected that the metabolism would be so much increased by the action of light that even a wholesale formation or substitution of phloroglucin in the briefest time would be accounted for. Thereupon Waage performed an experiment forthwith on the seeds of buckwheat, which indicated the probability of some though only a slight consumption of phloroglucin accompanying the rapid formation of the red colouration in etiolated seedlings which are suddenly brought into the light. It was also observed that in the

same cells where phloroglucin can be detected, tannin-reactions also take place; but the reverse was not the case, as many cells contain tannin without phloroglucin being present therein. On the whole, Waage concluded that phloroglucin takes part in the formation of phlobaphenes, tannin-reds, and, besides the tannin acid, it has a share also in the brown colouration of dead or dying autumn leaves.

Although the researches just mentioned were conducted mostly after the micro-chemical method, nevertheless their vast range and extraordinary minuteness of detail have supplied results which have proved of the utmost value to all practical students of this interesting theme. Nothing could be more suggestive than to be apprised regarding the precise distribution of phloroglucin, to know that it exists in one plant and does not exist in another, and sowise to lean gradually to the understanding that, although its function in the formation of certain red colouring matters stands pre-eminent, yet there are certain other red pigments with the origin of which it has got nothing whatever to do. It does not require a specially delicate eye for colour to see that the red of the rose is different from that of the foxglove, that of the rhododendron from that of the daisy, etc., and such being the case, it is reasonable to surmise or conclude that they have a different origin. What then does there remain for us to state or summarise with respect to the precise chemical causes which bring about the outcome of these brilliant red and blue phenomena of the vegetable kingdom? In a general way it may be admitted as established that, in Dicotyledons at least, where the red colour of flower or leaf verges towards bluish, as for instance in the rose and rosaceae generally, there the phloroglucin group enters into the molecule of the chromogen; where, on the other hand, the red is more scarlet, the chromogen seems to be of the nature of an acid, and the actual visible pigment is only a saline compound modified by exposure to the air and by certain substances gums, mucilages which accompany it and even possibly give origin to it. Both forms of tannic chromogen arise from a carbohydrate which, according to its chemical component and constitution, gives origin to certain intermediate tannoid compounds which seem to form, on the one hand, phloroglucin-tannin, and on the other, protocatechuic acid-tannins. The former class of tannins when oxidised or dehydrated produce the actual crimson colouration, and for this purpose they do not require to enter into combination with a base; the change seems to depend on what in chemical parlance is termed condensation, *i.e.*, the formation of an oxide or simple ether by the elimination of H_2O between two HO groups in different molecules. It has been surmised that the action is analogous to the action of aldehydes on phenols in the presence of dehydrating agents, as Lindt had originally stated (see above), but experiments conducted with phloroglucin, various

aldehydes, and HCl produced in every case a crimson resinous matter which was not turned green by alkalies and acetate of lead. This green reaction is however yielded by a substance which is obtained by heating the lead or zinc compound of the pure tannin of the horse-chestnut, etc., with dilute HCl, filtering off the phlobaphene and shaking the red filtrate with amyl alcohol, which dissolves a red substance which is washed with cold water and dissolved in ethyl alcohol. This seems to be the most successful attempt that has yet been made in the way of producing by synthesis the anthocyan and erythrophyll of flowers and leaves; and there seems little doubt that some such process as this is brought about in nature by the concentration of the cell sap through increased transpiration, etc., from these organs. It is as well to mention that, according to Jodin, light does not exert a photo-chemical action on tannin, although it does so on chlorophyll and carotin. Also that MM. Gautier and Girard experimenting on the regulated oxidation of the tannins came to the conclusion in 1877 that the red colouring matter of, *e.g.*, wine is only one of the transitory oxidation products of the oenotannin contained either in the skin or pulp of the grape. Tschirch likewise has pointed out that in the cell sap the tannins are oxidised directly (or, if they are glucosides, after resolution into their constituents) to red-brown phlobaphenes, some of which are formed very slowly, while others are formed very rapidly.

The latter class, viz., the protocatechuic acid-tannins, may be regarded as chromogens which evolve colouring matters not out of their own molecules by condensation and dehydration, but possess the property of forming coloured salts which turn blue, red, or even green when exposed to the air. It is possible, indeed, that they contain a salifiable group (chromophor) in their chemical structure; they are more entitled to be called tannic acids than the aforesaid class are. Their most important peculiarity, however, patent to everybody, is that they can embody a true blue flower. No one has ever seen, or ever shall see, a blue rose; but a blue dahlia or even a blue daisy is quite possible. The very fact of this possibility emanating, so to speak, from a source which is distinctly acid argues a power of fixation which is something stronger than that shown on the formation of a mere 'lake.' It was astutely observed by Stein that decidedly blue flowers, such as corn-flowers, contain a large quantity of calcium, phosphoric acid, and pectin. From experiments which I have recently made, I am much disposed to conclude that the presence of a certain kind of pectin associated with certain inorganic bodies explains the production of blue from the tannic chromogen, and likewise the fact that even in an acid medium the combination between these bodies is so complete that no separation or reddening occurs.

P. Q. KEEGAN.

SOME NEW BOOKS

"Parker and Haswell"

A Textbook of Zoology. By Professors T. J. Parker and W. H. Haswell. 2 vols. 8vo. Pp. 1400, with 1173 figs. London: Macmillan & Co., 1897. Price, 36s. net.

There appears to be no end to the making of Textbooks of Zoology, of one kind or another, to suit all sorts of readers. At one period Germany was manufacturing them one after another; at the present time the French zoologists are bringing out three or four textbooks, each on its own peculiar lines; and now it seems that we are to have a succession of similar productions by English authors. The generality of these last have been translations, or adaptations, or compilations, and if we except the "Cambridge Natural History"—which appeals, perhaps, to a different public—we have had no really original English textbook dealing with the entire animal kingdom in an adequate manner since Huxley's two small volumes on the anatomy of the vertebrata and of the invertebrata were published in 1871 and 1877 respectively.

It is, therefore, not surprising that "Parker and Haswell" has been looked forward to with expectation for some time. We welcome it gladly now that it appears: it should be in the hands of all students, and even teachers will find it of value. But a feeling of sadness and regret cannot fail to come over us, as we open the book, and remember that one of the two authors did not live to see the issue of his work.

The first volume deals with the invertebrata, the second with the vertebrata—in all there are 1400 pages and 1173 figures; and the craniata alone occupy 523 pages and are illustrated by 450 figures—which is sufficient to show the thoroughness with which the group is dealt with.

There is a good index at the end of each volume, and the whole book is attractively got up; the printing is clear, and the figures are mostly good, many original and some excellent. We are warned, in the preface, that we must not expect a very "up-to-date" book, for it is two years since the proofs were finally corrected by the authors, who lived 1200 miles apart, and were separated from their publisher by half the circumference of the globe.

The plan of the work reminds one of Huxley's once much-used Manuals. A 'type,' or, as our authors prefer to call it, "an example," of each class is described in detail; this is followed by the "characters" of the class, and of the orders and suborders into which the class is divided; the systematic position of the 'example' is then pointed out (too insufficiently to be of use, we think); and the general structure of the entire class is discussed; together with habits, distribution, and the relations of the orders and classes of the phylum.

There may be two opinions about this plan, but as the book is specially intended for a beginner, who has no previous knowledge of

zoology, it is probably the best suited for the purpose. After all, it is the plan followed in a general course of lectures on zoology, and the book becomes to some extent useful in the laboratory. But we wish that a greater uniformity had prevailed in the way in which the plan is carried out; for whilst in most cases, such as the Vertebrata, Arthropoda, Mollusca, a few representative genera are mentioned under each of the small divisions of the class, in other groups, *e.g.*, Annulata, Echinoderma, Coelentera, these are omitted, and the student will find it impossible to locate the various genera mentioned in the subsequent account of the general structure of the group as exhibiting modifications in this or that organ, as compared with the type.

Again, it would have added very considerably to the value and interest of the book, both to English and to Australasian students, if there had been uniformity in the plan of stating whether the 'example' is a Northern or a Southern form.

For instance, *Petromyzon* is taken as the type of the Cyclostomi; but we are told, in the opening sentence, that it is a Northern form, and is represented in the Southern hemisphere by *Geotria* and *Mordacia*. In the case of *Astacus*, however, we are merely informed that it is represented in the South by allied genera; but the fact that *Paranephrops* is the New Zealand genus is only mentioned in the chapter on geographical distribution at the end of vol. ii. [It may be noted that *Paranephrops* and *Asthenosoma*, and some other genera mentioned in the text, are not to be found in the index.] But in the Annulata and Echinoderma no indication is given as to whether the type occurs in Australasia or not; there is nothing in the immediate text to show that *Lumbricus*, the type, is represented by *Acanthodrilus* in New Zealand.

The classification adopted will, perhaps, scarcely satisfy specialists in the various groups; nevertheless, there is much to be said against the tendency, so frequent at the present day, of introducing new 'systems' into books which are, after all, intended as a guide to students. These new classifications are really stumbling-blocks to a beginner, for in reading, as he should do, other text-books or zoological classics, he will meet with different systems which he has not sufficient knowledge to co-ordinate with the more modern one which he has recently read. Moreover, the beginner is unaware of the instability of 'systems,' and naturally thinks that the facts of the science being firmly established (as he believes), the deductions from these facts should be stable also. Nevertheless, there comes a time when a rearrangement must be made, and such a sub-division as that of the Crinoidea into Neo- and Palaeo-crinoidea should be given up. So, too, the groups 'Myriapoda' and 'Gephyrea' ought to be split up, and the forms hitherto included distributed to their proper places.

We are glad that the authors refuse to see in the Polyclad Turbellarians any close relationship with the Ctenophora, but rightly place the Rhabdocoelida at the base of the class, and recognise that *Ctenoplana* and *Coeloplana* are merely slightly modified Ctenophores.

Dinophilus is placed with the Rotifera and Gastrotricha, as a phylum

Trochelminthes. In the account of the Nemertines no reference is made to the important difference in the position of the nerve cords with respect to the musculature in the various orders, that was pointed out long ago by Hubrecht. *Balanoglossus*, *Cephalodiscus*, and *Rhabdopleura* are placed, but with some hesitation, with the Chordata.

Amongst the novelties of the book may be mentioned the number of fossil vertebrates described and figured, the amount of embryology of each type, and the phylogenetic trees at the end of each Phylum. No doubt the last have their use, but one or two of them do not appear to agree with the text. Thus, on p. 483 of vol. i., the tree representing the Annulata, shows a 'chaetopod' stem rising from the Archiannelida, and ultimately bifurcating into Oligochaeta and Polychaeta. This is all right, but the Gephyrea and Hirudinea are seen issuing, one on each side, from this common stem. This does not indicate the, probably, true course of events, nor, indeed, does it agree with the statement that "the Hirudinea diverge somewhat widely from the Chaetopoda, but a study of their earlier developmental stages shows unmistakably their close connection with the latter group, more particularly with the Oligochaeta."

There can be little doubt but that the Leeches are degenerate Oligochaeta—for have we not a Leech with bristles?—and they should therefore have been represented as springing from that class and not from the common stem. The armed 'Gephyrea,' too, can be traced step by step to the Polychaeta.

The description of many of the examples will be found very useful, some because of their novelty as types, others as being local forms. Thus, *Triton nodiferus* represents the Gastropoda, instead of the snail; *Anthenea* stands for *Asterias*; *Chiloscyllium fuscum* replaces *Scyllium canicula*.

The anatomical account of *Apus* and of *Hormiphora* will be welcome, and especially interesting is that of *Callorhynchus*. But some of these descriptions lose in value owing to errors, both of commission and omission. The figure of the transverse section of *Nereis* is wrong in the limitation of the coelom; there is no mention of the 'ciliated organ' which was discovered and described by Goodrich in 1893, and is now known to be of considerable morphological importance. The authors clearly recognise that the Polychaete nephridium, or 'diplo-nephridium,' differs from that of the Oligochaete, which is an 'ectonephridium.' Goodrich's observations have a direct bearing on this distinction.

Is it true that "in *Nereis dumerilii* there is only a single pair of testes, situated in one of the segments between the 19th and 25th?" If it is (and we doubt it) it is so exceptional a limitation amongst the Polychaeta that it should be specially mentioned as such.

In the account of *Cucumerina*, we meet with the astounding statement (p. 372) that in the respiratory trees, "each of the terminal branches ends in a ciliated funnel opening into the coelome." Is there, here, a confusion with the sessile, ciliated cups (without external outlet) of *Chirodota* and *Synapta?* or with the posterior nephridia of Echinids? On p. 439, as well as in the account of the earthworm, the nephridial funnel is said to open into the coelune in the segment "corresponding to" the external pore. Surely everyone

knows the fact, peculiar as it is, that the nephrostome of Chaetopods is always in the segment preceding that of the pore—though the significance of this arrangement is unknown. These are samples of sundry errors; and others of less note occur in the descriptions of some of the types. If definite examples are taken to illustrate the structure of the group, it appears to us that every statement in connection with them should be as accurate as dissection and careful observation can make them. Some of the errors will be discovered by any student who dissects the type with any care at all. Also, we fail to see why the nephridium of the type (*Lumbricus*) is illustrated by a figure of that organ of *Tubifex*; it would have been quite as easy to borrow the block of Gegenbauer's figure of the earthworm nephridium, as that of Lang's.

On p. 569, the statement occurs that "in *Scutigera* one of the stigmata is unpaired and dorsal"; the fact is that in this Chilopod, they all have this position; no mention is made of the peculiar organ into which they lead. On p. 612, the abdomen of *Limulus* is said to consist of "seven firmly united segments." On p. 627, it contains "only eight segments"; but Lankester showed years ago that the abdomen consists of twelve fused segments.

In vol. ii., pp. 137 and 158, we read that the vertebrae of Selachians contain "bone"; this is a bad error, since calcified fibro-cartilage is meant, which is not the same thing as bone, either histologically, chemically or developmentally.

We have referred to the abundance and general excellence of the illustrations, but it seems strange that it is necessary to illustrate *Apteryx* by a photograph of a stuffed specimen in the Museum of the Royal College of Surgeons, London. The picture of the egg of *Callorhynchus*, with the embryo within, is especially good, and we are glad to see those of foetal marsupials with the embryonic membranes; there are good ones of Aurelia, Sponge anatomy; but one featue of the pictures is the diagrammatic dissection or solid section, *e.g.* of Amphioxus; the ideal vertebrate is also a striking picture.

But some err in the attempt to show too much; two or more less elaborate figures would have been clearer. Thus, the dissection of the Leech, fig. 373, does not give a true idea of the alimentary canal, since among other things the coeca are represented both in a state of distension and of contraction on the same side, and there is a figure (338) of seven bristles of different Polychaeta without any indication, anywhere, as to what genus each bristle represents. Some figures are attributed to wrong authors, or the author is not mentioned; thus fig. 269 is by Van Bemmelen, and fig. 259 by Allmann, fig. 165 is by Julin—these names are not mentioned.

An inexplicable error occurs on p. 188, vol. i. Fig. 137 is Lacaze Duthiers' well-known picture of the Red Coral, but it is described as "*Cirripathes anguinea*" (after Bronn). A curious transposition of the legends of two phylogenies occurs on pp. 580, 582 of vol. ii.

It is a notoriously difficult task for one, or even two, men to write a Text-Book which shall be free from errors, and satisfy the various specialists who may have to review it, for each is apt to consider his particular group as the group that should be properly done. The present book is no exception. Nevertheless, in spite of

several errors and omissions, the book will be found very good of its kind; it is so well illustrated, so clearly printed and generally good, that it will be found a useful addition to the student's and teacher's shelves.

Apart from the descriptive matter, special reference should be made to the chapters at the end of the second volume on the Philosophy of Zoology, the History of Zoology, and on Geographical Distribution, with especial reference to Australasian forms. These subjects are rarely read by beginners, and we commend these chapters to their consideration, and hope that, bound up as they are with the more 'paying' matter, they will spare some hours from their 'schools'' work to study the history of these subjects. There is one noticeable difference from most modern books, and that is an absence of literature in the body of the book. It may be true that it is impossible to give a complete or sufficient bibliography within reasonable limits, but it is possible to give a carefully chosen and representative bibliography, and it is advisable, we think, that a student, even a beginner, should be induced to read original memoirs. It is more stimulating than a Text-Book, and gives him a fuller knowledge of how the facts of the science are discovered and verified and correlated; and we ourselves have always found that beginners take a great deal more interest in zoology, if they can be induced to read occasionally special papers and memoirs.

In the "Guide to Literature" at the end of the present book the majority of references are to students' Text-Books of one kind or another; only a few monographs are referred to, and many important ones could have easily and appropriately been added. It is, however, a good idea to add a list of such periodicals as the *Zoologischer Anzeiger*, the *Centralblatts*, &c., wherever current literature may be found.

The Riches of Dresden

Guide to the Royal Collections at Dresden. Translated by C. S. Fox. Published by authority. 8vo, 290 pp. Dresden, 1897. Price, 1 Mark.

The number of English speakers, especially Americans, who visit Dresden every year, many of them living there for some time, has caused the authorities of the museums and picture galleries in that town to issue an English edition of their handbooks. We find herein an account of the Zoological, Anthropological, and Ethnographical Museum, which, although the successor of a cabinet of art and natural curiosities formed by the Elector Augustus, 1553-1582, practically dates from 1849, in which year the museum was burnt by the revolutionaries. Following this is a short description of the Mineralogical, Geological, and Prehistoric museum, which has had a similar history to that of the zoological collections, and is now placed in the west wing of the Zwinger building, the zoological collections being in the south wing. These guides are intended for *das grosse Publicum*, not for the scientific student: they do not, as do the guides of our British Museum, give any connected account of the objects contained, but content themselves with drawing attention to the more striking among them. They are probably no less useful to the visitors for whom they are intended, but do not form such interesting reading at home.

G. FISCHER, of Jena, announces the publication of a "Fauna Chilenis," based upon the collections made by Prof. L. Plate. The separate sections of the work are undertaken by some twenty-seven specialists, whose results will be published as supplementary volumes to Spengel's *Zoologischen Jahrbücher*, but will be obtainable separately at a somewhat higher price.

SERIALS

The Royal Botanic Gardens, Ceylon, have recently begun to issue a circular intended to deal with the agricultural, horticultural and botanical work there carried on.

The *Science Teacher* is a new monthly started in New York by Mr A. T. Seymour, instructor in science and mathematics in Westminster School, Dobbs Ferry, N.Y.

The *Philadelphia Medical Journal* commenced publication in January. The editor is Dr. Geo. M. Gould, and he is helped by many leading physicians of Philadelphia.

The New York weekly, *Garden and Forest*, is dead. It was founded about ten years ago by Prof. Sargeant of the Arnold Arboretum, and was edited by the late Mr W. A. Stiles.

The *American Archaeologist* (formerly *The Antiquarian*), published at Columbus, Ohio, contains useful and valuable matter dealing with the primitive peoples of North America in particular. This year it commences volume ii., and the subscription is $1.50.

The February number of *The Photogram* contains two articles of interest to our readers. One begins a series on Anatomy in Portraiture; the other is a short practical note by A. E. Livermore on a means of producing stereoscopic effects with Röntgen photographs.

The section of Archaeology and Palaeontology of the University of Pennsylvania has just begun the publication of a review entitled *The Free Museum of Science and Art.* It is intended to give an account of accessions to the museum, notes on the collections, and short scientific papers.

The following entry occurs in the contents on the wrapper of the *Journal* of the Society of Arts for 4th February, "Correspondence:—Recreations of an Indian Official—Dry Rot"! The "Recreations" contain, however, an interesting list of the native names of Indian plants by no less a person than Sir George Birdwood.

L'Intermédiaire des Biologistes (see *Natural Science*, January, p. 63) continues to fulfil the promise of its earlier numbers. The following questions are pretty certain to find answerers among our readers: **121**, What museums contain remains of either skeletal or soft parts of *Rhytina borealis?* **130**, Where and when did Mr F. Galton publish his last researches on heredity? Replies sent to us will be forwarded.

Messrs Munn & Co., 361 Broadway, New York, have kindly sent us a cloth-bound copy of their index to the more important papers

contained in the *Scientific American Supplement*. We often refer to the *Scientific American*, but we are not favoured with the *Supplement*. Messrs Munn tell us that it is "a unique journal, containing no advertisements." The issue of this index is a unique and useful way of advertising it. If every periodical followed the example of the *Scientific American* we then should accumulate a fine subject index of contemporary literature, as complete as that which Mr Cotgreave is trying to compile.

The *Westminster Review* for February has a charming bit of sarcasm in a review of a book on birds by a Dr W. T. Greene. It appears that he follows the classification adopted in the Gardens of the Zoological Society, not because he approves of it, but for the convenience of his readers. On this our contemporary remarks: "That classification is very unsatisfactory, and it would be a good thing if the society were to consult such an authority as Dr Greene, instead of leaving the subject in the hands of mere amateurs." Our eminent ornithologist, Dr P. L. Sclater, and that skilled writer on bird anatomy, Mr F. E. Beddard, will chuckle over this phrase. Unfortunately, people more ignorant than the Westminster reviewer are apt to take irony so very seriously.

We have received the first three livraisons of volume vii. of the *Actes de la Société Scientifique du Chili*. Dr. C. Pérez Canto has an article on the reproduction of certain bacteria from the point of view of their classification. Gustavo Undurraga contributes some detailed anatomical investigations of the pneumo-gastric nerve. E. Riggenbach describes a new species *Bothriotaenia Chilensis*, found in the intestine of a carnivorous gadoid fish living on the coasts of Chili, known as *Genyptorus Chilensis*. F. Gautier writes on the presence of antimony in certain tin minerals of Bolivia. F. Lataste discusses several remarkable cases of teratology, and contributes what he calls a study of rational biology, "Symmetry in Living Beings." There are also minor papers by F. Lataste and A. Dugès, and notes by Dr Otto Nordenskjöld on Patagonia.

Further Literature Received

Traité de Zoologie Concrète. v. Vermidiens, Delage et Hérouard: Schleicher, Paris. Elementary Botany, Groom: G. Bell. Elementary Physics, Kerr: Blackie. Die Gattung Cyclamen, Hildebrand: Lehrbuch der Entwickelungsgeschichte des Menschen, Kollmann; Organographie der Pflanzen, Goebel: Fischer, Jena. Andrée and his Balloon, Lachambre and Machuron: Constable.

The Gipsy Moth in America, Howard: *Bull. U. S. Dept. Agriculture*. On an Index to Literature, Cotgreave. Die Resultate der Tiefseeforschung, Chun. Catalogue of Zoological and Palaeontological Works, No xlii. Echinodermata., xliii. Vermes: Dulau. Catalogue of the Hadfield Collection of Shells: Manchester Museum. Anthribidae from the Islands of Engano, &c., Jordan: *Ann. Mus. Genova*. Studies from the Yale Psychological Laboratory, vol. iv.

Actes Soc. Sci. Chili, vi. 4, 5, vii. 1, 2, 3; Amer. Journ. Sci., Feb.; L'Anthropologie, viii. (6); Bol. Mus. Paraense, xi., No. 2; Botan. Gazette, Jan.; Feuille des Jeunes Nat., Feb., and Bibl. Cat.; Irish Nat., Feb.; Journ. Malac., vi. 304; Knowledge, Feb.; Literary Digest, Jan. 15, Feb. 5; Mem. Soc. Cient. Mexico, x., Nos. 5-12; Naturae Novit., Dec. No. 24, Jan., No. 1; Naturalist, Feb.; Nature, Jan. 20, 27, Feb. 3, 10, 17; Chivers' New Book List, Jan.; Photogram, Feb.; Proc. Biol. Soc. Washington, vol. xii. pp. 5-30; Revue Scient., Jan. 22, 29, Feb. 5, 12, 19; Science, Jan. 14, 21, 28, Feb. 4; Scientific Amer., Jan. 8 and 15, 22, 29, Feb. 5; Journ. School. Geogr., Jan.: Intermediaire des Biol., i. 2 and 5; Journ. Conchol., ix. No. 1; Rivista Psichologia, fasc. 18; Revue Sci. Nat. Ouest., April 1897; Scot. Med. and Surg. Journ., Feb.; Scot. Geogr. Mag., Feb.; Westminster Rev., Feb.

OBITUARIES

LIEUT.-COL. CHARLES COOPER-KING, of Kingsclear, Camberley, who died, aged fifty-four, on January 16th, was an enthusiastic exponent of the natural sciences at the Staff College, Sandhurst. From 1886 to the time of his death he was lecturer on applied science there, and drew a large class of officer-students to geology, both in the lecture-room and in the field, for being a military expert, his explanations of the science in relation to military tactics and battlefields were well appreciated. He also added to our knowledge of the geology of Berkshire and of the local prehistoric man.

DR ALBRECHT SCHRAUF, professor of mineralogy at Vienna, and for many years custos in the Imperial Museum of that city, died in December 1897, aged 60. His "Lehrbuch der physikalischen Mineralogie" (1868) and his "Atlas der Krystallformen" (1865-1877) are the most important among many important books and papers on mineralogical subjects.

THE death of Prof. RUDOLF LEUCKART, of Leipzig, is announced. We hope to give some account of his life and work next month.

The following deaths are also announced:—Dr WALDEMAR v. SCHROEDER, professor of pharmacology in the University of Heidelberg and author of a number of treatises on physiological chemistry; CESARE CRETY, professor of zoology and comparative anatomy in Sessari, Sardinia, on Sept. 14, 1897; Dr MAX ZEPPELIN, the zoologist of Stuttgart, on Dec. 3, 1897, aged 41; Rev. WILLIAM HOUGHTON, ichthyologist, at Wellington, Somerset; JAMES THOMSON, the entomologist, well known as the author of the "Systema Cerambycidarum," and other monographs on beetles; ALESSANDRO LANZILOTTI, professor of veterinary anatomy at Milan, aged 40; F. ALEX. SLUDSKI, professor of geodesy in the University of Moscow, on Nov. 25, 1897; FRIEDRICH ADOLF HOFFMANN, the geologist, in Mexico; Dr EUGEN ZINTGRAFF, the African traveller, on Dec. 4, 1897, aged 39; ALFRED MONOD, the cryptogamic botanist, aged 61; HENRY N. BOLENDER, botanist, at Portland, Oregon; and KASIMIR VON PIETROWSKI, killed during the botanical expedition of Tatra, aged 20.

NEWS

THE following Appointments are announced :—Prof. Michael Foster to be Gifford lecturer in Glasgow University for the sessions of 1898-99 and 1899-1900; F. C. Kempson, of Caius College, Cambridge, to be demonstrator of anatomy at the University; Dr W. B. Benham, Aldrichian demonstrator in comparative anatomy at Oxford, to be professor of biology in the University of Otago, N.Z.; W. G. van Name, to be assistant in biology, and G. L. Bunnell, assistant in zoology, at the Sheffield Scientific School of Yale University; Dr Rhumbler to be professor of zoology in the University of Göttingen; Dr Sigmund Fuchs to be professor extraordinarius of physiology in the University of Vienna; Dr Abelous to be professor of physiology at the University of Toulouse; R. H. Biffen, of Caius College, Cambridge, to be demonstrator of botany at the University; Dr Julius von Istvánffy, of Budapest, to be professor of botany in the University of Klausenburg; Dr Alexander Mágocsy-Dietz to be associate professor of botany in the University of Budapest; Dr Richard Klebs, of Königsberg, to be professor; Prof. O. Mattirolo, of Bologna, to be professor of botany and director of the museum and botanical garden at Florence; Prof. F. Morini to take his place at Bologna; Francis Ramaley, instructor in pharmaceutical botany at the University of Minnesota, to be assistant professor of botany in Colorado University, Boulder, Col.; Dr P. Zenetti, of the Pharmaceutical Institute of Strassburg University, to be professor of chemistry and natural history at the Dillingen Lyceum; Romul Alex. Prendel, of Odessa, to be professor of geology and mineralogy at Klausenberg; H. W. Pearson, of Christ's College, Cambridge, to be assistant curator of the herbarium of the University; Dr Otto Finsch, of Delmanhorst, to be director of the ornithological department in the Rijks Museum of Leyden; William J. Moenkhaus, of Indiana University, to be custodian of the Paulista Museum at San Paulo, Brazil; Frederic Philippi, professor of natural history and director of the Botanical Gardens at Santiago (Chili), has succeeded his father, the veteran Prof. Rudolph Armand Philippi, as director of the National Museum of Santiago, Prof. R. A. Philippi having retired at the age of ninety, after forty-three years' service.

DR ARTHUR WILLEY has been re-elected for one year to the Balfour Studentship in Biology at Cambridge University.

WE regret to learn that Sir J. William Dawson of Montreal is suffering from a stroke of paralysis and in a critical state of health.

MAJOR QUINCY of Boston suggests that £13,000 be appropriated immediately for the establishment of a marine aquarium in Boston.

PROF. J. O. EDMOND PERRIER has been elected *membre libre* of the Paris Academy of Medicine in place of the late Dr Magitot.

THE Hoagland Biological Laboratory of Brooklyn, N.Y., has received from its founder, Mr C. N. Hoagland, a mortgage for 24,000 dollars.

SIR NATHANIEL LINDLEY, Master of the Rolls, has been elected a Fellow of the Royal Society in virtue of his membership of the Privy Council.

THE Ragonot collection of Microlepidoptera and the Berthelin fossil Foraminifera are among recent additions to the Museum of Natural History at Paris.

THE Field-Columbian Museum of Chicago has purchased the complete herbarium of the late M. S. Bebb, which is specially rich in specimens of *Salix*.

THE Société d'Acclimatation of Paris intends to issue, in addition to its *Bulletin*, a monthly journal which will be largely devoted to discussion and correspondence.

THE U.S. Government has sent Mr B. E. Fernow, chief of the Division of Forestry, to Hawaii to make preliminary explorations and a report on desirable forestry legislation.

MR GEO. K. CHERRIE, assistant curator of ornithology in the Field Columbian Museum, has resigned his position in order to explore the region of the Upper Orinoco.

PROF. WM. LIBBEY, of Princeton University, intends to celebrate the annexation of Hawaii by taking four of his students there on a scientific expedition during the coming summer.

MR CLEMENT WRAGGE, the founder of Ben Nevis Observatory, and now Government meteorologist of Queensland, proposes to establish an observatory on the summit of Mt. Kosciusko.

PROF. O. C. MARSH has presented his unique palaeontological and osteological collections to Yale University, in which he has been honorary professor of palaeontology for thirty years.

PROF. H. A. MIERS has been granted the sum of £50 a year for five years to assist in the purchase of specimens and apparatus for the mineralogical department, Oxford University Museum.

ON December 18, 1897, a hall was opened at Bologna for the reception of the herbaria, preparations, and sections of the botanist Aldrovandi. It has been erected at the cost of the city and province.

THE Cambridge Archaeological and Ethnological Museum has received from Mr W. W. Skeat, District Magistrate of Larnt, Perak, a large collection of Malay native objects of great interest and importance.

THE Romanes lecture this year will be delivered in the Sheldonian Theatre, Oxford, on June 1, by Sir Archibald Geikie, who has announced as his subject, "Types of Scenery and their Influence on Literature."

IN accordance with the wish of the late Prof. John Tyndall, Mrs Tyndall has forwarded to the Royal Institution a cheque for £1000 to be disposed of as the board of managers may see fit for the promotion of science.

THE Rev. George Henslow, who has been appointed professor of botany to the Royal Horticultural Society, has undertaken to give demonstrations on the plants exhibited at a number of the meetings during the present year.

MR GEORGE SHARMAN, palaeontologist to the Geological Survey, retired at the end of last year, having served since 1855. We understand that he will be succeeded by Mr F. L. Kitchin, a graduate of Cambridge and Munich.

GOVERNOR BLACK of New York proposes the purchase by the State of a forest tract for the purpose of scientific cultivation, the administration to be in the hands of the Regents of the University or the Trustees of Cornell University.

MR JONATHAN HUTCHINSON, whose museum at Haslemere, Surrey, is well known, intends to establish an educational museum of a similar nature in his native town of Selby, Yorkshire, using for it the overplus material from Haslemere.

Prof. F. Noll of Brünn, the literary executor of the late Julius Sachs, Professor of Botany at Würzburg, is now working over the unpublished manuscripts of the latter. An admirable account of Sachs by Prof. Noll, with a portrait, is given in *The Botanist Gazette* for January.

At its meeting on 12th January, the American Academy of Arts and Sciences elected John M. Coulter, of Chicago, and Douglas H. Campbell, of Palo Alto, as Associate Fellows in the Section of Botany, and Elias Metschnikoff, of Paris, as Foreign Honorary Member in the Section of Zoology and Physiology.

On the 29th January the Horniman Museum at Forest Hill ceased to be open to the public for a time. Mr Horniman intends to pull down the present building and erect a more suitable one as a memorial to Her Majesty's Jubilee. It is hoped that the foundation stone of the new building may be laid in June.

The Museum of Rouen is being reorganised, and we learn from *La Feuille des Jeunes Naturalistes* that it is spreading into the building previously occupied by the School of Design. It will now be possible to display in a worthy manner the fine collection illustrative of Normandy geology formed by Mr Bucaille.

During 1897, says the same journal, all the specimens of the Noury ornithological collection in the museum at Elbeuf have been revised by Mr L. Coulon, who has published a catalogue of them through the Société d'Etudes des Sciences Naturelles of that town. In the same museum the Lepidoptera, especially those of Normandy, have been completed by Mr Dupont, while Mr Lancelevée has classified the Formicidae. Mr Raoul Fortin has been classifying the Quaternary rocks and fossils, while the Gastropoda have been revised and labelled by Mr Lhomme.

Prof. Guido Cora has resigned the professorship of geography at the Royal University of Turin, held by him for the last sixteen years, that he may devote himself to scientific research in geography and the allied sciences. His address, and that of his periodical *Cosmos*, will in future be 2 Via Goito, Rome.

The Director of the U.S. Geological Survey has been instructed by Congress to prepare a map of Alaska, showing all known topographical and geological features, including gold-bearing rocks. The text, which will state the best known routes to the gold-fields, will also be issued. 40,000 copies are to be printed.

It is interesting to learn, as we do from *Science*, that there is a scientific position in the U.S. Department of Agriculture, for which only women are eligible. It is the post of assistant microscopist to the Department. The microscopical inspection service has of late been greatly extended, and vacancies are to be filled in sixteen different cities.

Prof. K. Mitsukuri, the eminent zoologist of Tokyo, who has recently been in Washington as Japanese Plenipotentiary to the congress on the seal question, passed through England in February. He intends visiting Paris, Naples, and various German centres of learning, and hopes to be able to return for the Zoological Congress at Cambridge.

We learn that the Fossil Plants at the British Museum, which have for years been divided between the Geological and the Botanical Departments, have now been merged into one collection. This is only one of many enlightened movements which have taken place recently in the Museum, and the new arrangement will be of especial benefit to botanical students.

Experience has not accustomed us to associate the Roman Church with Science in any other position than that of antagonism. We are the more pleased to learn that, on the initiative of the Marquis de Maroy, of Wassy, a natural

history museum is being established at the Vatican. Geological and mineralogical collections are already displayed in a large gallery.

It is stated that Captain Otto Sverdrup intends to take the *Fram* northwards up Smith's Sound, and then to make sledge expeditions over the northern part of Greenland. £1111 (20,000 kroner) has been voted by the Norwegian Storthing for the purpose of making such alterations in the *Fram* as will give more room for scientific work and render her more seaworthy.

A temporary post of observation erected on the peninsula of Rase Tarfa, opposite Massowah, by the Austro-Hungarian warship "Pola," engaged on a scientific exploration in the Red Sea, under the direction of Dr Franz Steindachner, was suddenly attacked by Bedouins on January 11. The attack was repulsed without loss by the personnel of the post, aided by the Turkish camp guard. The station was then broken up.

L'Anthropologie informs us that Count H. de la Vaulx has returned to Paris from Patagonia with valuable anthropological and ethnographic collections made throughout the whole of that vast country, and comprising both ancient and modern specimens. Among other things may be mentioned more than a hundred human skulls and skeletons. These collections have been offered to the French Government, and will be studied by the officers of the Museum of Anthropology.

At the recent meeting of the American Society of Naturalists, there was founded a new Society for Plant Morphology and Physiology. Abstracts of the papers read are given in *Science* for January 28. The officers for the ensuing year are:—President, W. G. Farlow; vice-presidents, J. M. Macfarlane and G. F. Atkinson; secretary-treasurer, W. F. Ganong. The next meeting of this Society will be held in December 1898, in conjunction with the American Society of Naturalists and the Affiliated Societies.

Among the sums given by the Swedish Government in aid of scientific research, we note the following:—To Dr C. A. Westerlund, to aid the publication of his work, "Synopsis molluscorum extramarium regionis palaearcticae," 700 kronor; Mr N. O. G. Nordenskjöld of Upsala, to aid in the publication of the scientific results of the Swedish expedition to Magellan Straits of 1895-97, 1000 kronor; to Baron A. E. Nordenskiöld in return for 30 copies of his work, "Periplus," described by us in our last number, 4500 kronor.

At the annual meeting of the Geological Society of London on February 18th, Mr William Whitaker succeeded Dr Henry Hicks as president, and Mr W. W. Watts succeeded Mr Marr as secretary. The following awards were made:—The Wollaston Medal to Prof. Ferdinand Zirkel, and the Fund to Mr E. J. Garwood; the Murchison Medal to Mr T. F. Jamieson, and the Fund to Miss Jane Donald; the Lyell Medal to Prof. W. Waagen, and the Fund divided between Messrs H. Woods and W. H. Shrubsole; and the Barlow-Jameson Fund to Mr E. Greenly. At the annual meeting of the Geologists' Association on February 4th, Mr J. J. H. Teall succeeded Mr E. T. Newton as president.

A scientific congress is to be held at Buenos-Aires on April 10-20 of this year, to celebrate the 25th anniversary of the foundation of the Société Scientifique Argentine. Among the sections are those of Biology, Anthropology, and Geology, including Mineralogy and Palaeontology. Subscriptions, 5 pesos (about £1), and announcement of one's wish to attend, should be sent to the President of the Committee of organisation at Cevallos, 269, Buenos-Aires. We have given some account of the vast riches of the La Plata Museum in the pages of *Natural Science*, and we can add from personal experience the assurance that any of our European colleagues who attend the meeting will meet with a hearty welcome.

THE *Scottish Geographical Magazine* quotes *Globus* to the effect that the Danish Commission for Geographical and Geological Researches in Greenland has received 150,000 kroner from the Karlsbergstiftung, to be devoted to the exploration of the east coast from Angmagsalik to Scoresby Sound. A party will be carried from Denmark in the autumn by a trading vessel to Angmagsalik, and will travel northward as far as it can reach before the vessel returns in the following year. On its report will depend the plan for the main expedition which in the summer of 1900 will be landed at Scoresby Sound, and having passed the winter there, will make its way southwards. Lieutenant Amdrup of the Danish Navy is the appointed leader.

THE Secretary of State for War has replied to the petition of the Guildford Natural History Society in reference to the preservation of Wolmer Forest as a sanctuary for wild birds, thus constituting a national memorial of Gilbert White of Selborne. In his reply, the Marquis of Lansdowne states that, while he is fully in sympathy with the aims of the memorialists, he considers the continuance of the existing system would, so far as it is in the power of the War Department, best promote the objects of the Society. Enclosed with the reply was a copy of a report by Captain A. H. Cowie, hon. secretary of the Aldershot Game Preserving Association, who has had the Government ground in question under his management since 1895. This report stated that since that date all birds had been strictly protected.

THE trustees of the Elizabeth Thompson Science Fund met in Boston, Mass., on January 13, and made the following grants among others :—$150 to Prof. J. M'K. Cattell, for the study of fatigue in relation to mental conditions. $250 to Prof. J. von Kennell, for a Monograph of the palaearctic Tarteicidae. $25 to Prof. Wm. Z. Ripley, for a Bibliography of the Anthropology and Ethnology of Europe. $100 to Prof. C. H. Eigenmann, for the study of Blind Fishes. $250 to Prof. P. Francotte, for the investigating of the fecundation and segmentation of the eggs of Polyclada. New applications will be considered in January 1899, provided they are received by the secretary before December 1, 1898. Circulars announcing the terms of the trust for the guidance of applicants may be obtained by application to Prof. C. S. Minot, Harvard Medical School, Boston, Mass.

ALTHOUGH local natural history societies abound in the north of our country, are organised, combined, and do good work for science, yet they do not seem to flourish with such vigour in the south. For this reason we are the more pleased to learn of the healthy condition of the Portsmouth and Gosport Natural Science Society, especially as, only four years ago, it was almost given over for dead. At the twelfth annual meeting on January 12, it was announced that the number of members was fifty-three, and it is hoped that the present year will see a material increase, and with this some more practical scheme of organised work. At present the Society confines itself to hearing lectures on the first and third Wednesdays of each winter month, and to making outdoor excursions during the summer. The president for the current year is Dr Charles Foran, a leading Southsea dentist and ardent naturalist, and the secretary is Mr Edgar L. Curtis, 57 Victoria Road S., Southsea.

CORRESPONDENCE

MR COSTE ON DARWINISM AND DESIGN

THE problem of keeping up the proper intercommunication between the various branches of human knowledge is one of such increasing intrinsic difficulty as the sciences grow more specialised, that we should be disposed to welcome rather than to disparage any effort in this direction. For nothing (except the work itself) contributes more essentially to the soundness of work in each science than that it should keep in touch with the rest, and from time to time stop to contemplate its work from the standpoint of another science. Yet any attempt to determine the general value for knowledge of a special branch of research is apt to be regarded by the specialist as an intrusion upon his domain rather than as a demonstration of intra-scientific amity. This is a pity, but I might have anticipated that my attempt to throw some light on the logical status of the Darwinian Theory, in an article on "Darwinism and Design" published in the *Contemporary Review* for June 1897, would expose me to misrepresentation and hostility, and I was prepared to bear a certain amount of this with philosophic composure. But I cannot help thinking that Mr F. H. Perry Coste would have done better service to his own cause if his criticism of my paper in the December number of *Natural Science* (vol. xi., p. 408), had not been so largely composed of irrelevancies interspersed with the refrain "Yah, metaphysician!" The more so, that this appeal to an obsolescent prejudice is as little a proof of Mr Coste's perspicacity as of his good temper. For even if the whole tenor of my article did not show it to be intended as a piece of philosophical criticism, he had my own word for it (*l. c.* p. 871) that I only claimed to be a philosopher.

I take the more pleasure in drawing attention to a passage where Mr Coste has read me aright because in so many other cases he gives me much ground to complain of his ostentatious disregard of my stated purpose.

Mr Coste criticises—or perhaps rather abuses—me severely for not doing a variety of things which it lay beyond the scope of my article to deal with. I do not discuss, he urges, (*a*) "the evolution of the solar system" (p. 409); (*b*) I do not prove that variations are inherited or in fact definite (p. 411, 413); (*c*) I ignore Weismannism (p. 414); (*d*) I do not disprove all the mechanical alternatives to teleology (p. 413); and (*e*) I do not prove a God (!) (p. 413). It is very terrible that all these things should have been left undone in an article of eighteen pages! But I may perhaps be permitted to point out that I very distinctly disclaimed the intention of doing any one of these things, and that it is scarcely candid on Mr Coste's part to ignore these disclaimers. I rigidly limited myself to the subject indicated in my title, *viz.*, Darwinism and Design, for the very good reason that no one can profitably discuss everything at once. Moreover I had it in mind to follow up my criticism of Darwinism by papers dealing with the other evolution theories, although the pressure of other work has necessarily postponed the execution of this project. And even Mr Coste will hardly deny that a criticism of modern biological conceptions must start with Darwinism, and not with the latest lucubration of Prof. Weismann. Still, if it is any consolation to Mr Coste, I am quite willing to state with respect to the several omissions laid to my charge, as to (*a*) that I have yet to learn that Darwinism has any thing to say about "the evolution of the solar system"; as to (*b*) that I regard this as a proper question for the biologist and said so (*l. c.*, p. 871, 879); as to (*c*) that it was not incumbent on me in dealing with Darwinism to discuss Prof. Weismann's latest modification of a theory which, as Mr Romanes showed (in his "Examination of Weismannism"), has long ago been forced by the pressure of the facts to give up the pretensions to logical consistency with which it at first impressed even philosophers; as to (*d*) and (*e*) that my purpose was only the humbler one of showing that "properly understood Darwinism was not necessarily hostile to teleology" (*l. c.*, p. 882). Obviously the *proof* of such contentions would require one or more volumes. But I thought it valuable to clear the ground for such proof by showing that Darwinism was not inconsistent with teleology. If I had gone on to show the same thing in the case of the other theories of Evolution, and succeeded, I should then have been in a position to make felt the positive argument for teleology. This argument is philosophic in character, and, in my opinion, irrefragable and sufficient to make

teleological explanation the ideal of human science, even though our science should not now be (nor ever be) in the position to realise it in practice. But my article manifestly attempted nothing so gigantic, though I trust it did not exhibit the deplorable blindness to the complexity and scope of the questions at issue which appears in Mr Coste's criticism.

In addition, however, to condemning me for not putting into my discussion what it would have been mere folly to include in it, Mr Coste accuses me of various confusions of language and so charges me with 'metaphysical sophisms' and 'fallacious trifling with words.' This is, of course, a particularly discouraging reply to an honest effort to insist on the distinction of ideas ordinarily confounded. It can, however, I think, be easily shown that in this case the confusion of terms (and of thoughts) is Mr Coste's and not mine. I am said to "write as though evolutionism and Darwinism were the same thing." I challenge Mr Coste to find a passage in which I do anything of the sort; while I could show him many where the two are implicitly and explicitly distinguished (*cp.* especially the first and last paragraphs of my article). Such a confusion would indeed be strange in one who has long been conscious of being an evolutionist without thinking Darwinism the Alpha and Omega of Evolution.

Mr Coste, on the other hand, clearly succumbs to the popular fallacy of taking the most prominent species as coextensive with the genus in at least two passages. (1) After quoting a passage in which I had spoken of "Darwinism *qua* Darwinism," he proceeds to say (p. 413) that if I had "possessed any acquaintance with Weismann's work," &c. This surely involves a fusion of Darwinism with Weismannism, which, in spite of the dependence of Weismann on Darwin, must be regarded as a very serious confusion. (2) A little later Mr Coste complains that "metaphysicians who want to write about evolution will not take the trouble to find out what evolutionism connotes at the present day." Now I was writing about Darwinism, which (in my ignorance of "what evolutionism *connotes* at the present day"!) I had imagined to be only *one* out of the many possible theories about Evolution; whereas Mr Coste here equates Darwinism with evolutionism in a manner which seems to me destructive of all clear thinking. But perhaps 'what evolutionism connotes' in Mr Coste's eyes is equivalent to 'what it is popularly and inaccurately confused with.'

Again, I am accused of paltering with a double sense of 'adaptation.' The word apparently = 'inherited structural adaptations' (p. 411). Well, if that were true, one could only say that (like the 'elliptical' sense of denial of design mentioned on p. 410) it is not a good specimen of scientific precision of language. But in reality my 'fallacy' consisted only in proving that in a general sense the possibility of 'active adapting' (*l. c.*, p. 869) could not be disputed. This was established by adducing the active adaptings of conscious beings, and whether these result in inherited structural adaptations or not is totally irrelevant. That is, Mr Coste fails to see that the point at issue is whether 'adaptation' is wholly mechanical or also purposive, and confuses it with the wholly irrelevant question whether purposive adaptations are inherited or only functional! If any one mixes up distinct senses of words, it is not myself.

Lastly, I must allude to a very puzzling exhibition of Mr Coste's logic. On p. 410 he seems to consider it absurd that I should have been at pains to show that a completely logical working-out of Darwinian assumptions might deny the efficacy of intelligence as such, and reduce all animals to automata. This simply shows his ignorance of the length to which materialistic explanation can go and has gone, but does not make it less necessary for me to prove at the outset that "intelligence, *i.e.*, action directed to a purpose, has been at work." Which, accordingly, I did (*l. c.*, p. 871). Yet on the next page I am told that I have 'not even attempted' the proof of this proposition! And a little later we hear that 'we all know' that this intelligent adaptation is a fact (in which case surely it would have been superfluous to 'attempt' a proof!).

These specimens of Mr Coste's ratiocination go far to re-establish my conviction that there is still room for philosophic criticism in science, even more than his admission (p. 412-3) of the value and novelty of my contention as to the methodological character of the Darwinian assumptions. I had, indeed, anticipated that this would prove to be the most interesting part of my argument for the biologist; but even here Mr Coste fails to state its full import, which lies in the inevitable corollary (*l. c.*, p. 880-1) that the real inconsistency is not between Darwinism and design, still less between teleology and the facts of organic history, but between design and an abstract application of the calculus of probabilities.

F. C. S. SCHILLER.

MATHEMATICAL BIOLOGY

THE mathematical treatment of biological problems involves a certain danger; we have seen that about a year ago (see *Nature*, vol. lv. p. 155), and we see it again in the case of Mr Vernon's theory of "Reproductive Divergence."

The first premise of Mr Vernon's theory (see *Natural Science*, vol. xi. p. 405) expressed in an algebraic formula is as follows:—

		Small.	Medium.	Tall.
Parents: 100 SS	give offspring	$a.\times$	$b.\times$	$c.\times$
100 MM	,,	$b.\times$	$d.\times$	$b.\times$
100 LL	,,	$c.\times$	$b.\times$	$a.\times$

$$\text{I.}—(a+b+c).\times=(2b+d).\times=(a+b+c).\times;$$

a, b, c and d being the percentage numbers of offspring produced by the intermarriages between S with S, M with M, and L with L under normal conditions; $\times=\frac{100+m}{100}$ being the measure of fertility under those new conditions as assumed by Mr Vernon.

The second premise of the theory (*l.c.*) is that the numbers of offspring produced by the intermarriages of short and medium, and of medium and tall parents, "may be approximately obtained by taking means between the percentages for short and medium parents on the one hand, and for medium and tall ones on the other." Hence we have:—

		Small.	Medium.	Tall.
Parents: 100 SM + 100 MS	give offspring	$(a+b).y$	$(b+d).y$	$(c+b).y$
100 ML + 100 LM	,,	$(b+c).y$	$(d+b).y$	$(b+a).y$
100 SL + 100 LS	,,	$2b.z$	$2d.z$	$2b.z$

$$\text{II.}—(a+2b+c).y+2b.z \mid (2b+2d).y+2d.z \mid (a+2b+c).y+2.b.z;$$

$y=\frac{100-n}{100}$, and $z=\frac{100-p}{100}$, being the measure of fertility of the intermarriages between different parents.

The number of medium offspring will be smaller or larger than that of small and tall ones, or equal to this number, if we have—

$$(2b+2d).y+2d.z \begin{matrix}>\\<\end{matrix} (a+2b+c).y+2b.z\text{; or, as in (I.) } a+b+c=2b+d,$$

$$d \begin{matrix}>\\<\end{matrix} b;$$

that means, the answer to the question whether there are more medium than small individuals under II. is entirely independent of x, y, z, or of the degree of fertility resp. sterility, but depends solely on the percentage number of small (b), medium (a), and tall (b) offspring produced by M intermarrying with M under ordinary circumstances. Mr Vernon must so alter his premise I., that of the offspring of M marrying M less than one-third are medium individuals ($d<b$)! This, however, cannot be a premise of "Reproductive Divergence," as it would mean putting the cart before the horse.

If this simple mathematical demonstration should not be intelligible enough, Mr Vernon will perhaps see the fallacy in his theory, if he takes that degree of sterility between different parents, which should be the most favourable one for the theory, namely, absolute sterility not only between the extremes S and L, but also between S and M, and between M and L. In this case we have to do only with the outcome of the marriages of S with S, M with M, and L with L; *i.e.*, only with the numbers under I., which are equal, as Mr Vernon says himself (*l.c.*).

KARL JORDAN.

ZOOLOGICAL MUSEUM, TRING,
February 12, 1898.

DIPELTIS AN INSECT LARVA

I AM much obliged to Mr C. J. Gahan for directing attention to the fact that Dipeltis bears close relationship with certain coleopterous larvae. A larva almost identical with the one figured in the January number of this review was recently shown me by Mr E. A. Schwarz of the U.S. National Museum. Since seeing this specimen I agree with Mr Gahan's conclusions that *Dipeltis* is the larva of some insect, and not one of the Apodidae. The "two small shallow pits, which are interpreted as ocelli," and the "two faintly preserved eye spots," need be no longer explained as ocelli and eyes, since in the above-mentioned larva very similar modes and depressions are present, Mr Schwarz thinks the smaller specimen figured by me (figs. 4 and 5) may be related with the larva of Lampyridae or Dascyllidae. It is more natural to interpret the three anterior large segments of *Dipeltis* as divisions of the thorax of a Lampyrid larva than that they are parts of the cephalon of an *Apus*-like crustacean.

CHARLES SCHUCHERT.

U.S. NATIONAL MUSEUM.

THE AUTHENTICITY OF PLATEAU MAN

THE cudgels on behalf of the Authenticity of Plateau Man have been taken up by so many scientists that I only find it necessary to call attention to one paragraph of Mr Cunnington's article in the November number of *Natural Science* (vol. xi., p. 327). He states: "The vast number of the flint implements from the plateau gravel is another difficulty. . . . We are told that two pits dug in 1894 into a bed of gravel one foot in thickness yielded thousands of artificial flakes and some hundreds of scrapers."

Mr Cunnington is under a serious misapprehension here. It was not from the pits, but from the floor on the face of the Chalk Escarpment, nearly 80 yards long, that these numerous flakes were discovered. Moreover, these flakes were not those of Plateau man at all, but of the late Palaeolithic man, and are of undoubted authenticity, being of the ordinary form and bearing that hall mark of man's handiwork, the bulb of percussion. Many have been sent to and acknowledged by Sir John Evans himself.

If Mr Cunnington had carefully read the report to the British Association, Ipswich, 1895, he would have seen that in reference to the plateau gravel of one foot in thickness, my words were: "This gravel was hard and compact. From it I secured very many worked implements." Specimens forwarded at the time to Sir Joseph Prestwich brought the reply: "Interesting, rude but true." BENJAMIN HARRISON.

IGHTHAM, *Feby.* 6, 1898.

SOCIÉTÉ PHILOMATHIQUE DE PARIS

In a copy of the publications of this Society, preserved in the British Museum (Natural History), there is a portion of vol. i. entitled *Bulletin de la Société Philomathique à ses correspondens*, which is paged 1'-119'. As I have had a good deal of trouble about the date of this portion of the work, I may mention that in vol. iii. of *Bulletin des Sciences de la Société Philomathique*, p. 192, under the heading "Avis," there is a note which enables us to fix the date of publication of these pages as 1803.

C. DAVIES SHERBORN
(*Index Animalium*).

NOTICE

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